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A STUDY OF RETINAL SUMMATION

BY LEE EDWARD TRAVIS AND RALF MARTIN

University of Iowa

Introduction.—The purpose of the present study was to see if the visual acuity of two separate and distinct retinal areas, one in the temporal and the other in the nasal half of the same eye, stimulated simultaneously, was greater than the visual acuity of either one of these areas stimulated separately. The phenomenon of binocular summation is well known. To our knowledge, it is not known whether there is summation effect produced when two different retinal areas of the same eye are stimulated simultaneously. In attacking this problem the way we did, namely, by testing one retinal area having cortical representation in the one cerebral hemisphere and another retinal area having cortical representation in the other cerebral hemisphere, we thought we might throw some light on the bigger problem of the interaction of the two cerebral hemispheres. Before any definite conclusions may be drawn in regard to this bigger problem, it will be necessary to compare two different retinal areas having cortical representation in the same cerebral hemisphere. This is our next problem.

Apparatus and Procedure.—The apparatus is illustrated in Figs. 1 to 5 inclusive. The centers of the appertures for displaying the stimuli were 12.34 cm on either side of the fixation point. Each apperture was approximately 2 cm square. The fixation point was a white disc 0.74 cm in diameter. In Fig. 6, the stimuli are shown as actually used.

The exposure of the stimulus patterns was accomplished by the mechanisms shown in Figs. 2, 3, 4*A* and 4*B*. Two small armatures, *S* and *R*, each of which was activated by two electromagnets, *N* and *M*, and *L* and *K* respectively (Fig. 2), controlled the dropping of the stimulus slides, *B* and *A*, which moved in grooved guides, *G*. As an armature was pulled back and forth, it released and stopped, alternately, a stimulus slide by disengaging and engaging brass pegs, accurately spaced in a zigzag manner, on the slide's back. Each of the circuits that activated the armatures, *S* and *R*, contained

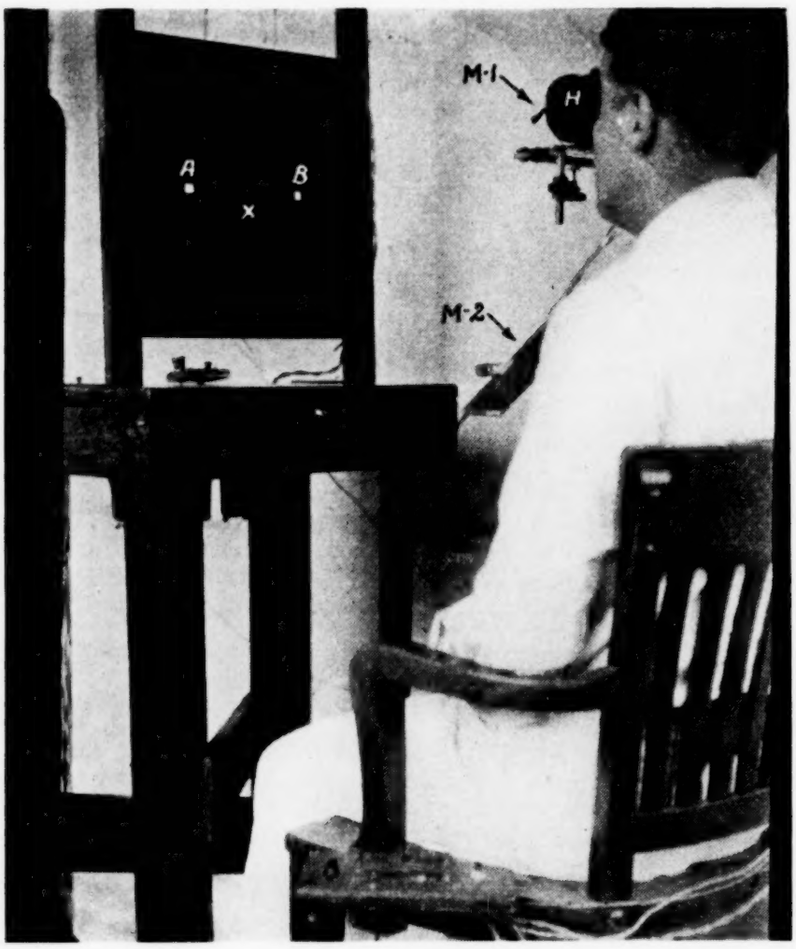

FIG. 1. Front view of the apparatus with subject in position for testing. *A* and *B* = stimulus appertures; *X* = fixation point; *H* = stereopticon frame; *M*-1 and *M*-2 = mirrors for observing movements of the eye not being tested; and *S* = experimenter's key to control stimuli and shutter disc.

a circuit that activated armature *F* through electromagnet *E* so that when either one of the former circuits was closed the latter circuit was also closed. When armature *F* was moved upward, the shutter disc revolved to pass an opening across in front of a stimulus pattern. The size of each opening and the speed of movement of the disc were such as to expose each stimulus pattern for 75 sigma. The shutter disc was accurately tooled from very light metal and placed between the stimulus pattern and the appertures *A* and *B* (Fig. 1), approximately 1/16″ in front of the former and flush with the latter. The disc had four equally spaced curvi-linear parallelogram openings

close to its circumference. Each opening was ½″ × 2½″. To the back of the disc was fastened a hard rubber pulley carrying weighted strings for keeping the disc under constant tension. The disc was moved through a distance equal to ¼ of its circumference each time the electromagnet drew the armature momentarily away from one of the four pegs on the back of the pulley. All electromagnets were supplied with a six volt direct current. When only one side of the retina was being stimulated, the armature and slide on one side of the apparatus were removed.

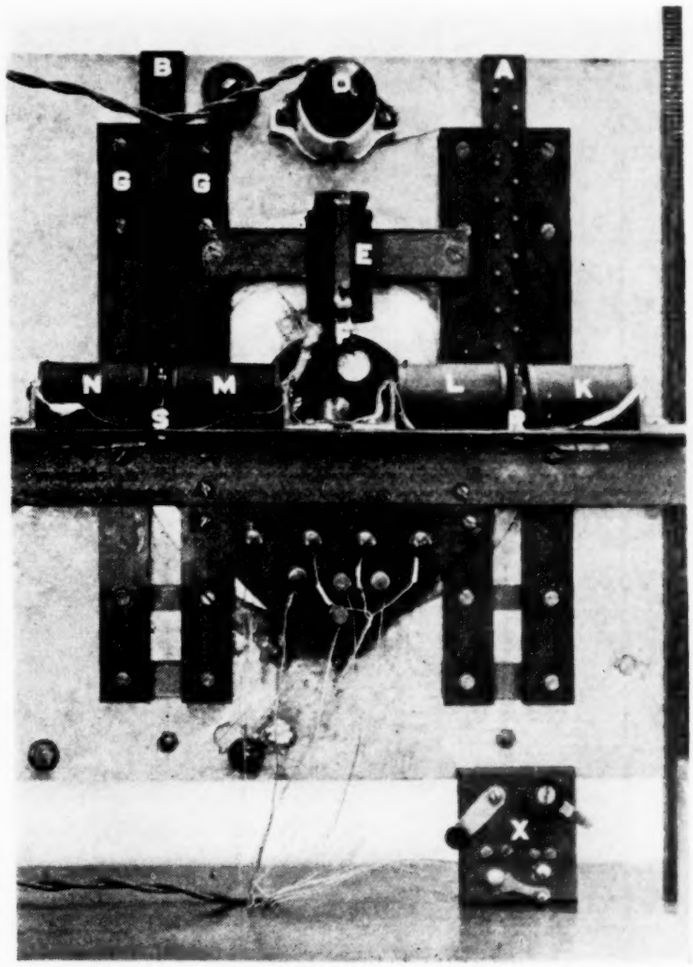

FIG. 2. Rear view of the stimulus and shutter disc mechanism. *A* and *B* = stimulus slides; *G* = grooved guides for the stimulus slides; *O* = red light bulb for operation of the apparatus in the dark; *E* and *F* = electromagnet and armature respectively for controlling movements of the shutter disc; *N*, *M* and *S* = electromagnets and armature controlling stimulus slide *B*; *L*, *K* and *R* = electromagnets and armature controlling stimulus slide *A;* and *X* = switch controlling all lights.

The stimulus patterns and the fixation point were illuminated by three 3.8 volt flashlight bulbs, one bulb at each of the stimulus appertures and one at the fixation point. In order to give a soft, mellow light, each bulb was supplied with only 1.5 volts direct current. Because the illumination of one stimulus pattern was the same as that for any other stimulus pattern, a change in density of the stimulus was brought about

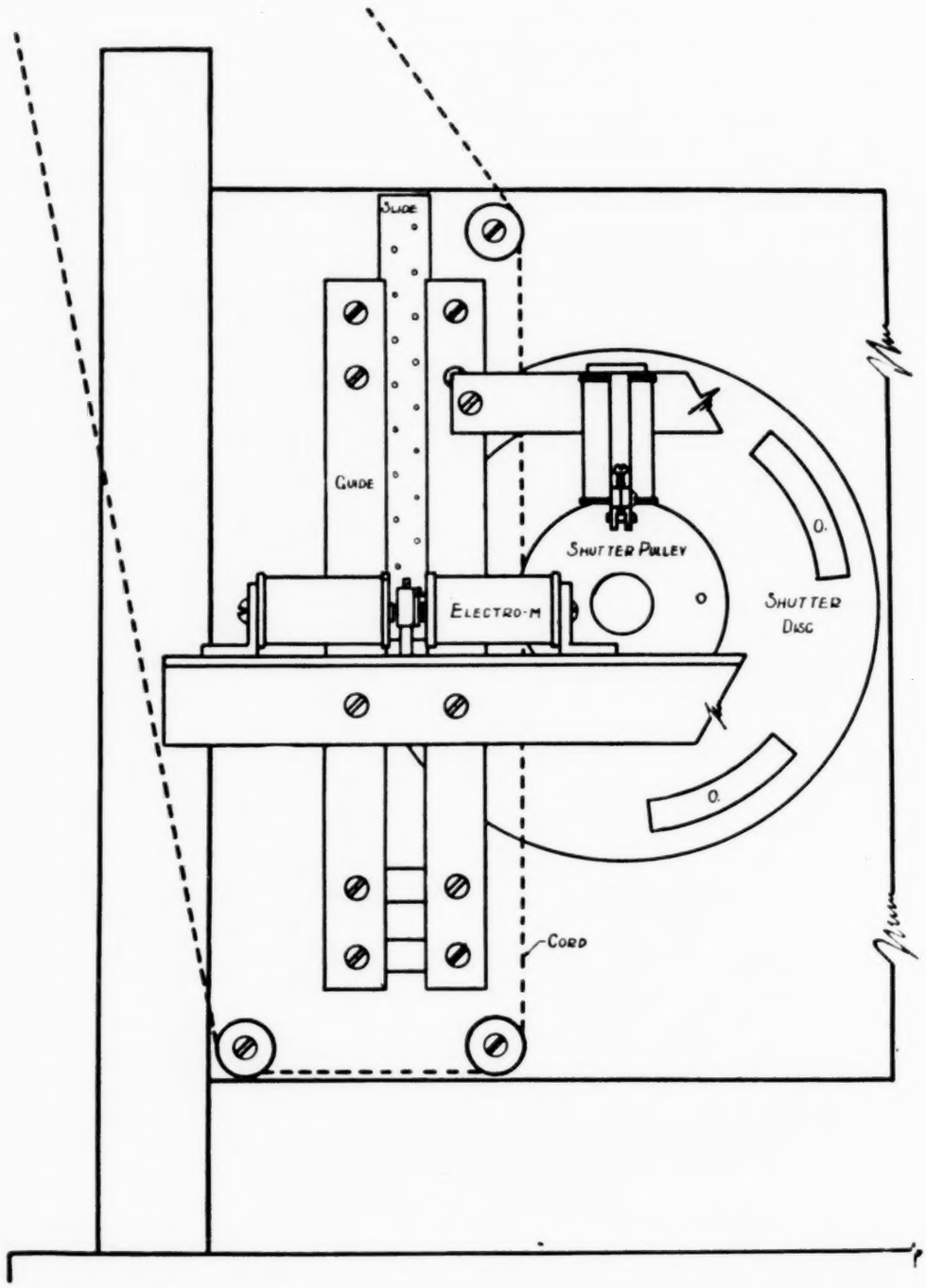

FIG. 3. Diagram of the mechanism controlling the stimulus slide and the shutter disc.

by the nature of the stimulus patterns themselves (Fig. 6). Mounted in a frame the whole apparatus could be adjusted vertically to suit the height of the subject.

The subject sat so that the cornea of the eye being tested was 60 cm directly in front of the fixation point. To maintain rigidly this position the subject was required to rest the head against a stereopticon frame adjusted each time to give exactly the required distance between the cornea and the fixation point. Each subject was instructed to look directly at the fixation point *X* (Fig. 1), and not to permit his eye to

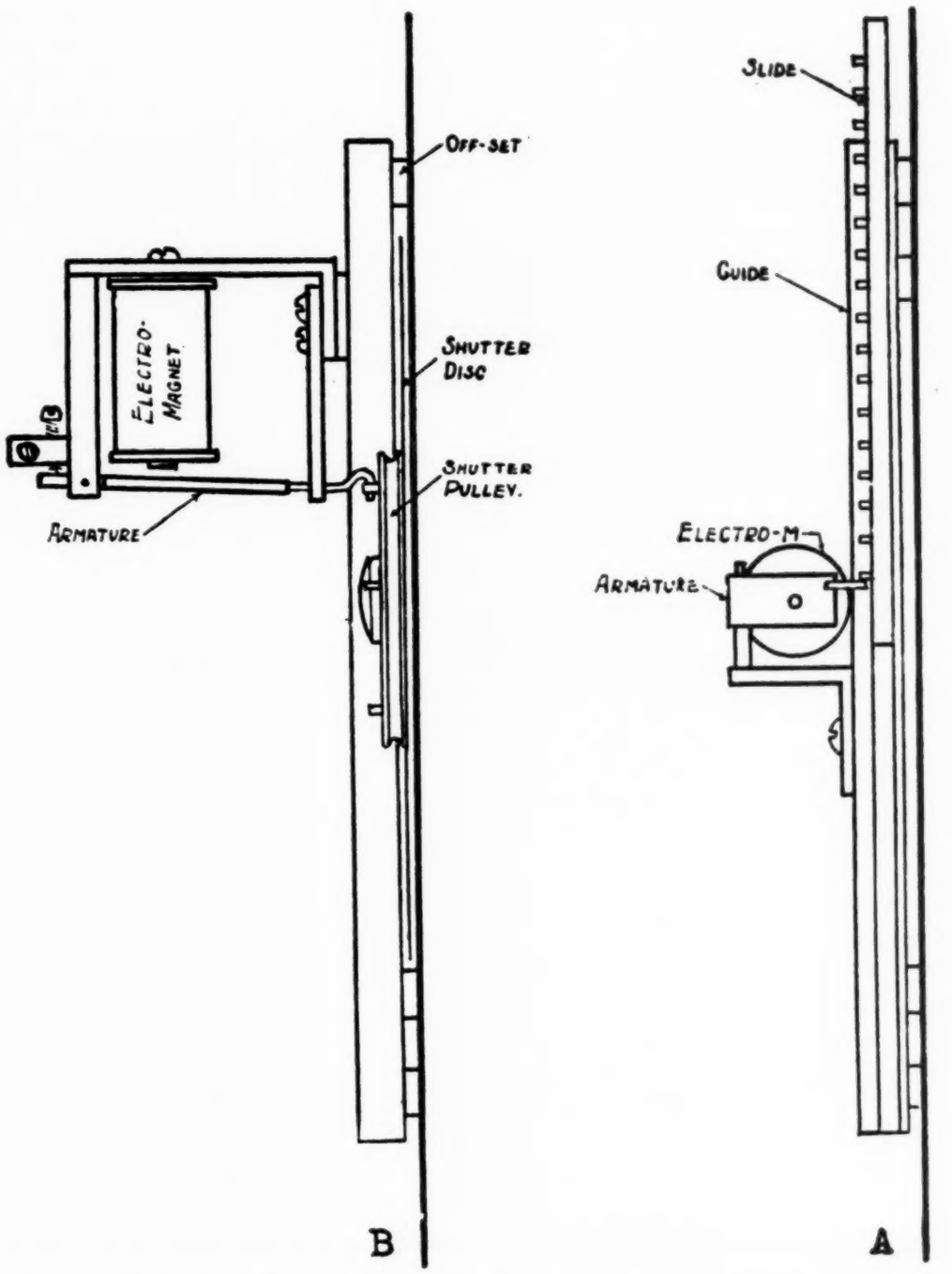

FIG. 4. Diagrams showing side views of the stimulus slide mechanism *A* and of the shutter disc mechanism *B*.

fixate directly the stimulus pattern at *A* or *B* (Fig. 1). However, to have an accurate and objective check on the subject's fixation of the fixation point, the eye not being tested was observed through a series of two mirrors by the experimenter (Figs. 1 and 5). A dim light illuminated this eye and furnished an image through the two mirrors so that the experimenter could sit to one side of the observer and note accurately whether or not the latter shifted his fixation point. The light for illuminating the control eye was so arranged that it did not affect the eye being tested.

In reporting the stimulus patterns the subject was asked to determine whether the lines were directed across, up and down, up to the right, or up to the left. He was told that the stimulus patterns were to be seen out of the corner of his eye. Each subject was run through two identical series of experiments, the first used as the practice, and the second as the final test series. The two series were done on different days, usually successive. For the practice series, the non-dominant and for the final test series the dominant eye was used. The dominant eye was determined by a modification of the

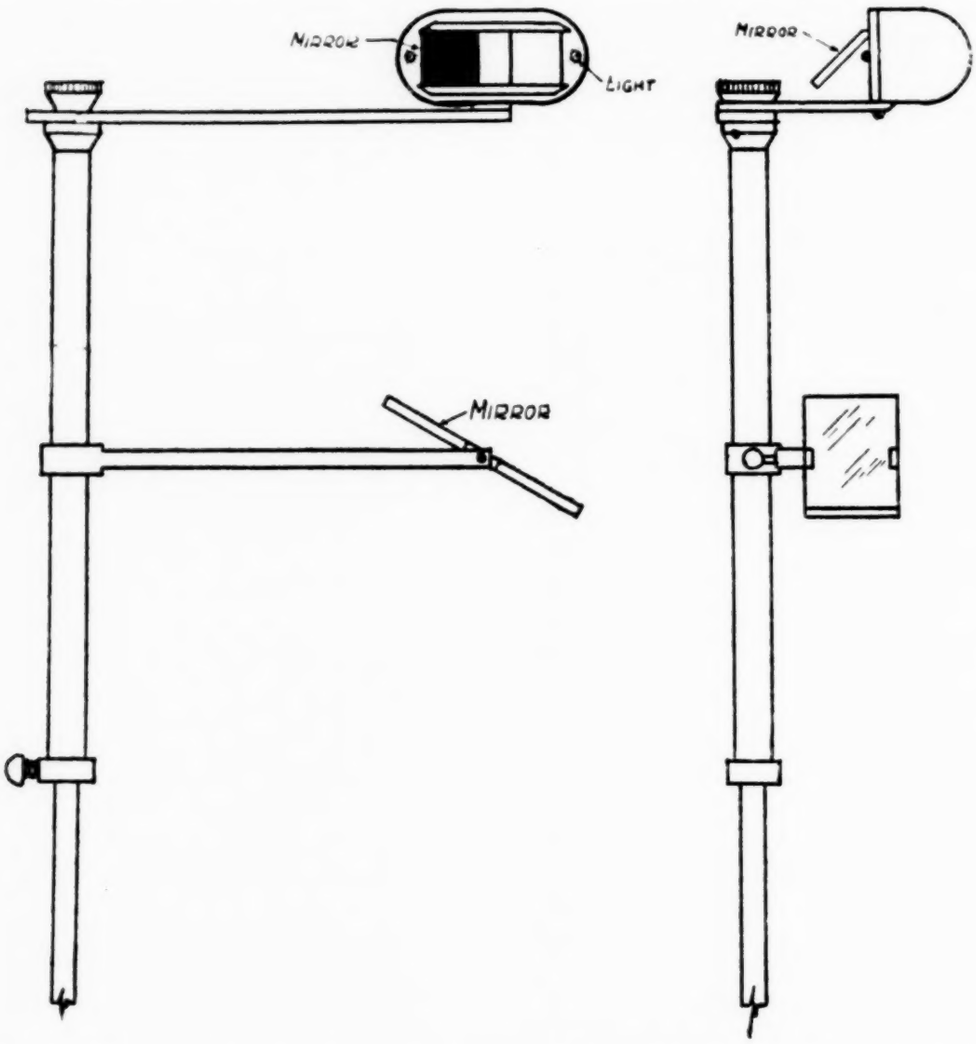

FIG. 5. Diagrams showing the mechanism used for an objective check on eye movements of the subject.

Parson technique which employs the principle of unilateral sighting in binocular vision (3).

All experiments were conducted in a dark room with scotopic vision after a five minutes period of dark adaptation.

Pathological astigmatism and the percentage vision were determined by means of the Green Grid Chart A. All of our subjects had normal vision (with or without glasses) and were relatively free from pathological astigmatism. If the subject's eyes were corrected with glasses, he was permitted to wear the glasses during the experiment.

In any given series of experiments, the general procedure was to test the right half of the retina first. This would be the nasal half for left eyed observers and the temporal half for right eyed observers. Following this, the left half of the retina of either the right or left eye, as the case might be, was tested, and finally both halves of the retina of either the right or left eye, as the case might be, were tested simultaneously. For each of these three tests the stimuli were presented first in the order of increasing density and second in the order of decreasing density. For testing both halves of the retina simultaneously, the two stimulus patterns were identical.

In order to check possible practice effects, this general order of procedure was varied

FIG. 6. The stimulus patterns.

in certain cases. Testing the two halves of the retina simultaneously would be medial instead of final in chronological order. This check indicated that the possible practice effect as here tested was inconsequential.

Because we wished to measure the sensitivity of a part of the retina which had cortical representation in one hemisphere and another part of the retina which had cortical representation in the other hemisphere, it was necessary to have the visual or optical angle of such a size as to make sure that the stimuli did not fall on the macula lutea. Also we did not want the stimuli to fall, in the case of the nasal retina, on the

optic disc (blind spot). By having the inner margins of the stimuli 8.54 cm, and the cornea of the eye being tested 60 cm from the fixation point, the optical angle thus formed was such that the inner edges of the stimulus patterns fell 2 mm on either side of the outer edge of the macula lutea, if the macula lutea is 1 mm in diameter, and 1 mm on either side of the macula lutea, if it is 3 mm in diameter. The macula lutea varies in diameter from 1 to 3 mm (4). These points may be clarified by reference to fig. 7.

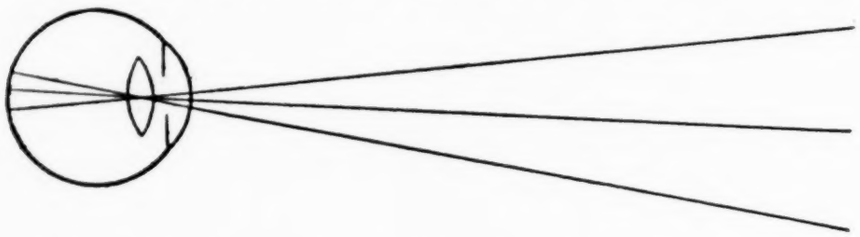

FIG. 7. The optical angle used in the study. The various dimensions are the same as those for the average eye.

Although from our calculations, the stimulus patterns did not fall distinctly in the field circumscribed by the optic disc, our results, as will be reported later, indicated that the influence of the optic disc might not have been entirely eliminated.

Results.—Fifty healthy adult individuals served as subjects in this experiment. There were 22 male and 28 female observers. They gave a mean age of 24.88, in a range of from 18 to 42 years.

This study showed that when the two retinal areas, one in the temporal and the other in the nasal half of the same eye, were stimulated simultaneously they gave greater visual acuity than when either one of them was stimulated separately (Tables I and II and Figs. 8 and 9).

TABLE I

MEAN NUMBER AND RANGE OF ERRORS AND THE PERCENTAGE SUMMATION FOR THE THIRTY-TWO STIMULUS PRESENTATIONS

	Nasal	Temporal	Average of Nasal and Temporal	Both	Percentage Summation
Mean.....	15.18	14.56	14.87	11.68	22.96
Range.....	6–24	6–25	7–23.5	5–21	0–65.51

TABLE II

MEANS AND RANGES OF THRESHOLDS (THE HIGHER THE NUMBER THE LOWER THE THRESHOLD, FIG. 6)

	Nasal	Temporal	Both
Mean.....	10.78	10.60	12.72
Range.....	6–15	5–15	8–16

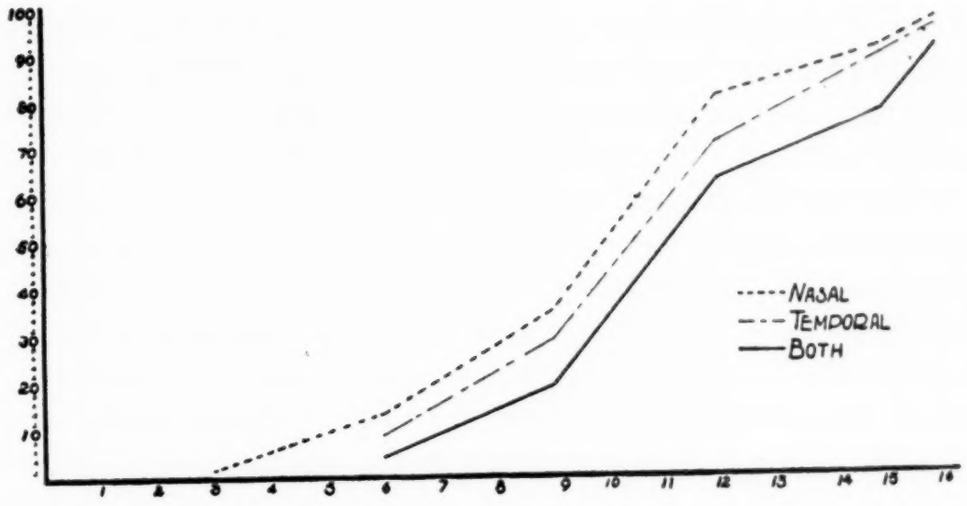

FIG. 8. Curves showing the percentage of all the subjects missing a given stimulus pattern for the nasal and the temporal areas stimulated separately and for both areas stimulated simultaneously. The stimulus patterns are grouped according to density.

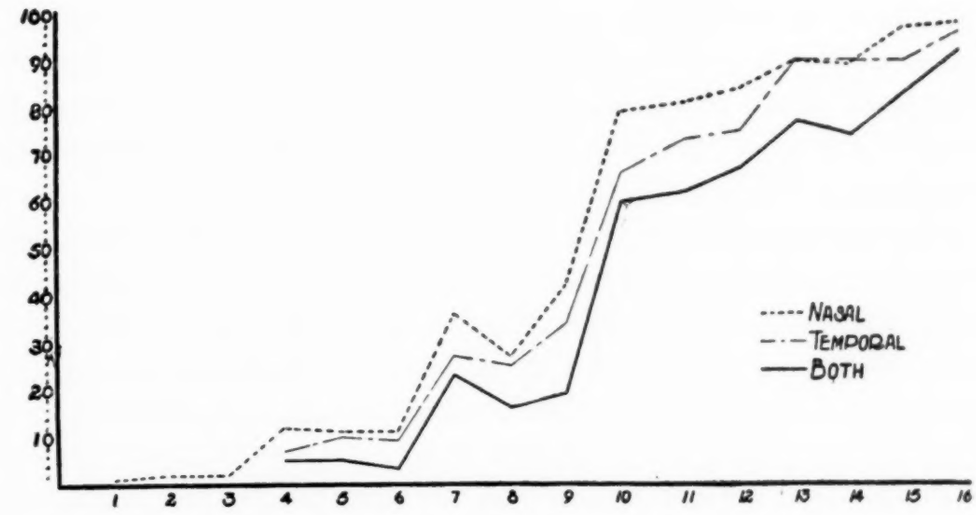

FIG. 9. Same as Fig. 8 except each of the 16 stimulus patterns is considered separately.

Of all the subjects, 70 percent made less errors for both areas than for either area, 12 percent made more errors for both areas than for either area, 4 percent made the same number of errors for both areas and each area, 2 percent made more errors for both areas than for one of the areas but an equal number of errors for both areas and the other area, 2 percent made more errors for both areas than for one area but less errors for both areas than for the other area, 4 percent

made the same number of errors for both areas and one area but more errors for both areas than for the other area, and 6 percent made the same number of errors for both areas and one area but less errors for both areas than for the other area.

The mean number of errors for the nasal and the temporal halves considered together was 14.87, and for the two halves stimulated simultaneously, 11.68 (Table I). The difference between these means is 5.43 times its probable error. The superiority of the two areas stimulated simultaneously over either area stimulated separately is better shown probably in Figs. 8 and 9. In these figures, the percentage of all the subjects missing a given stimulus pattern for the nasal and the temporal areas stimulated separately and for both areas stimulated simultaneously is shown. In fig. 9 each of the 16 stimulus patterns is considered separately, whereas in fig. 8, the stimulus patterns are grouped according to density as follows: 1 to 3 inclusive, 4 to 6 inclusive, 7 to 9 inclusive, 10 to 12 inclusive, 13 to 15 inclusive, and 16 alone (see Fig. 6). Fig. 8 presents no overlapping whatsoever of the curves. In terms of errors, the curves rank in the order of both areas, temporal area, and the nasal area.

By reference to Table II it may be seen that the visual threshold is lower for both areas stimulated simultaneously than for either area stimulated separately. The difference between both areas and the nasal area is 6.06 times its probable error and the difference between both areas and the temporal area is 7.00 times its probable error. Fifty-eight percent of the cases had a lower threshold for both areas than for either one.

There appears to be a tendency for the area tested in the temporal half of the retina to be more sensitive than the area tested in the nasal half of the retina of the same eye. Of all the subjects, 56 percent made fewer errors for the temporal area, 34 percent made fewer errors for the nasal area, and 10 percent made an equal number of errors for the two areas. However, the difference between the mean number of errors for the temporal area and that for the nasal area (Table I) is only 1.10 times its probable error. Further, in terms of the

threshold (Table II) the nasal area is slightly superior to the temporal. However, the difference between the thresholds is smaller than its probable error. Figs. 8 and 9 indicate that the temporal may be really superior to the nasal area.

This apparent difference between the temporal and the nasal halves could not have been due to brightness differences of the stimuli. For right-eyed subjects the stimulus at *A* (Fig. 1), stimulated the temporal retina while for left eyed subjects it stimulated the nasal retina. Inasmuch as there was approximately the same number of right and left eyed subjects, it appears that the tendency toward a superiority of the temporal over the nasal retina is not a question of technique.

Discussion.—In a consideration of our results, the question of the cortical representation of the areas we stimulated is an important one. Did they have cortical representation in the two hemispheres, one retinal area in one hemisphere and the other retinal area in the other hemisphere, or did they have cortical representation in two different areas of the same hemisphere? Fuchs (**1**) and Howell (**2**), among others, state that a line drawn through the middle of the macula lutea divides the retina into two halves, the nerve fibers from one half going to one hemisphere and the nerve fibers from the other half going to the other hemisphere (Fig. 10). If this be true, we did actually stimulate a retinal area with cortical representation in one hemisphere and another retinal area with cortical representation in the other hemisphere (Fig. 7). A line drawn from the inner edge of a stimulus pattern through the nodal point of the lens to the retina strikes the retina 2 mm from the edge of the macula lutea, if the latter is 1 mm in diameter, and 1 mm from the edge of the macula lutea, if it is 3 mm in diameter. In either case, it appears that we stimulated retinal areas, one area of which made connections with one hemisphere and the other of which made connections with the other hemisphere. The summation effect which we found for stimulating the two retinal areas simultaneously might be explained on the basis of the interaction of the two cerebral hemispheres thus affected. However, the localization in the

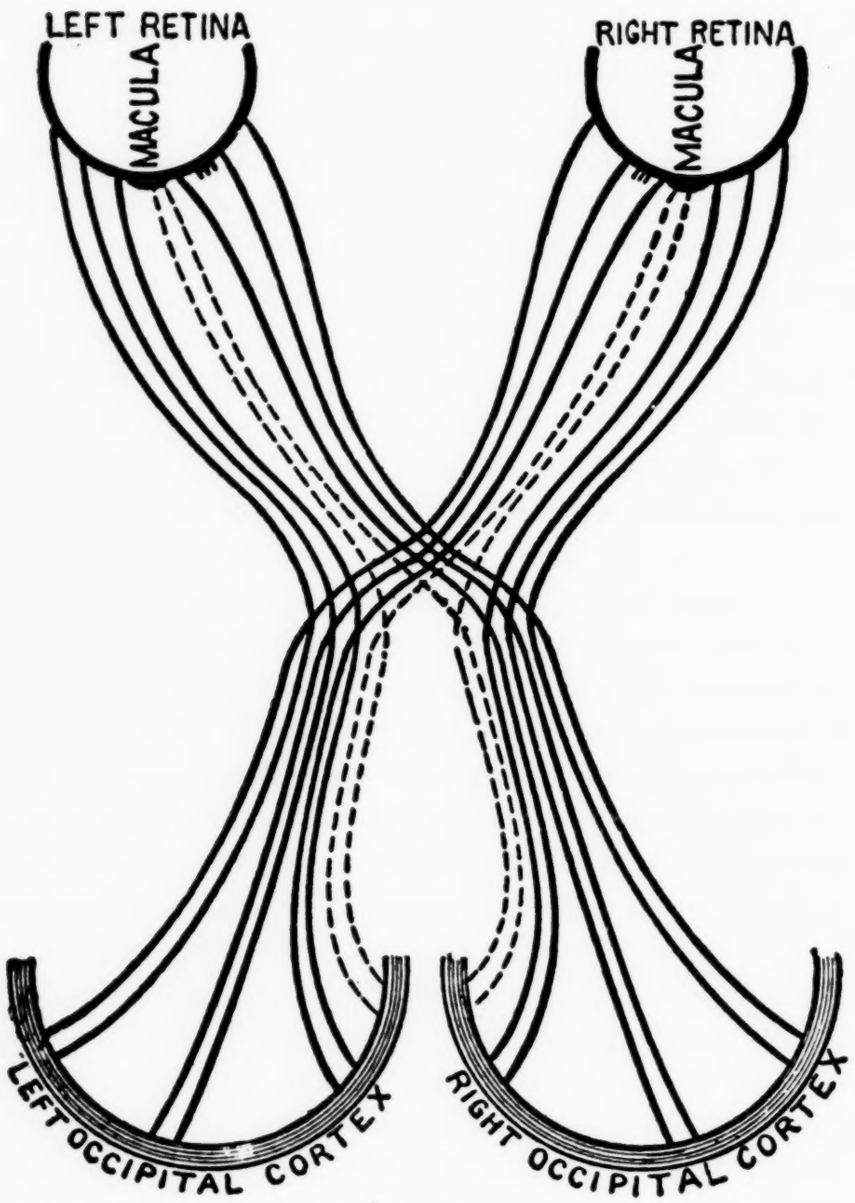

FIG. 10. Diagram showing the probable connections between parts of the retina and the visual area of the cortex. (From Howell after Schäfer.) The bilateral representation of the fovea centralis is indicated by the course of the dotted lines.

periphery (retina) of the summation effect cannot be ruled out entirely. It remains to see if the simultaneous stimulation of two retinal areas having cortical representation in the same hemisphere gives the summation effect. If it does, then we cannot explain our results entirely on the basis of the interaction of the two cerebral hemispheres.

We are not able to explain adequately the indicated

superiority of the temporal over the nasal half of the retina. It occurs to us, however, that the optic nerve's entrance into the nasal half might have affected our results. Although we were reasonably sure that our stimulus patterns did not fall directly on the blind spot, they might have fallen on areas closely circumjacent to it. In this case, we might get a reduction in retinal sensitivity here.

A relatively few subjects made more errors and had a higher threshold for both halves of the retina stimulated simultaneously than for either half stimulated separately. Although we do not know the real reason for this, a certain amount of light was thrown on it by the introspective reports of the subjects. Invariably, those subjects who gave the exceptions, reported that they could not see both stimulus patterns when these stimulus patterns were presented simultaneously. They made their decisions in regard to the direction of the lines in the stimulus pattern from one stimulus pattern or the other and not from both. With these subjects it appeared that the simultaneous presentation of two stimulus patterns acted as a deterrent rather than as a help. Whenever a subject turned out to be an exception to the general rule, he was tested again. In all cases, the second test gave the same results as the first one. Those subjects who gave summation for the two halves of the retina stimulated simultaneously reported that they saw both stimulus patterns clearly, although one of the stimulus patterns might be more distinct than the other.

Summary.—1. The purpose of the present study was to see if the visual acuity of two separate and distinct retinal areas, one in the temporal and the other in the nasal half of the same eye, stimulated simultaneously, was greater than the visual acuity of either one of these areas stimulated separately. Fifty normal, adult individuals served as subjects in this experiment. The sexes were approximately equally represented.

2. The two halves of the retina stimulated simultaneously were distinctly superior to either half stimulated separately.

3. There was a possibility that the temporal half of the retina was superior to the nasal half.

4. The entrance of the optic nerve into the nasal half of the retina might help account for the nasal retina's inferiority.

5. The superiority of the two halves stimulated simultaneously over either half stimulated separately might be evidence of interaction of the two cerebral hemispheres.

(Manuscript received August 31, 1933)

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AN EXPERIMENTAL STUDY CONCERNING VISUAL LOCALIZATION IN THE HORIZONTAL PLANE

BY WINFORD L. SHARP

College of Wooster

Under normal conditions one can with considerable accuracy locate a visual object as being in the same horizontal plane with his eyes. It seems evident, however, that this ability is dependent in large degree upon the use of environmental cues. The question comes: If these commonly used cues are excluded and one be forced to depend altogether on the kinesthetic cues from the ocular muscles will he still exhibit the ability to any considerable degree? Actually this is pretty much the situation in which the air pilot finds himself, particularly in night flying.

Hoppeler (1) published a study in 1913 in which he gave an answer to this question. Using twenty subjects of various ages he found that in general they saw the point of light as level with their eyes when in fact it was several degrees below such level. He reports the limits of their subjective horizons as being from 2 degrees above to 5 degrees below.

The writer, interested in this same general problem, has been working at it as opportunity offered throughout the past four years. This paper reports investigations in which answers to three questions were sought: (1) With what accuracy do individuals localize visual objects as in the plane horizontal with the eyes, when the usual external cues are excluded? (2)Does practice in this particular function, without knowledge of results, bring improvement in the function? (3) Does the tilt at which the head is held materially alter the accuracy of the localization?

Apparatus.—The nature of the problem necessitated the use of a dark room with a small point of light as the visual object. Two simple pieces of apparatus were needed: first, a device for exhibiting a small point of light at various levels and so arranged that

the light could be lowered or raised as S wished; second, a suitable device for holding S's head in the desired position with his eyes at a known level.

To meet the first need a device, which for convenience has been termed a 'homolometer,' was constructed. This consists essentially of an upright standard constructed of two pieces of lumber in such a way that a track is provided in which a small carriage may slide. By means of a simple windlass arrangement this carriage can be moved perpendicularly through a distance of 120 cm. It carries a small metal light-proof box which contains a fifteen watt bulb. A small point of light is emitted through a circular hole 2 mm in diameter, the intensity of the light being materially reduced by several thicknesses of frosted glass. A short strip of metal, serving as an indicator, is fastened rigidly to the side of the carriage in a position exactly opposite the point of light. This indicator passes directly in front of a long strip of record paper which is conveniently secured to the upright standard. By this means the experimenter can, even in the dark, accurately record the setting which S perceives as level with his eyes. With the true level marked on this strip it is then an easy matter to measure the magnitude of the error of perception.

To meet the second need a substantial platform was made; height 40 cm, width 70 cm, and length 100 cm. Toward the front of this platform at a convenient height for a seated person a narrow bar-like shelf, bearing a screen or shield with suitable aperture, is mounted. Instead of being rigidly fixed in the horizontal plane it is so mounted that it can be turned about a horizontal axis through an arc of some 20 degrees. This shelf serves as a head rest and by use of it S's head can be so adjusted that his eyes are at a known level, coincident with the axis upon which the shelf turns. With measures of S taken at the beginning, the tip of the chin and nose being used as reference points, it is possible to reproduce the normal head posture of each S and likewise, if desired, to alter the angle of tilt at which the head is held.

These two pieces of apparatus were set up in a perfect dark-room, the observation platform being so placed that S's eyes were 148 cm above the floor level and 240 cm distant from the homolometer. Some of the earlier work was done with S at a distance of 300 cm, but when forced to move into a smaller room the distance had to be reduced to 240 cm and most of the work was done with this set-up. An accurate spirit level was used to ascertain the point on the homolometer scale which was level with S's eyes. Once this point was marked it was permanent for all Ss of course.

Subjects and Procedure.—Forty-two Ss, college students from Denison University and the College of Wooster, had part in the work. Eight of these served in the preliminary work required for standardizing procedure; the records of thirty-three are herein reported; and the record of one had to be excluded since his responses were so consistently beyond the limits of the apparatus that valid results could not be had.

With the head measures carefully taken S was seated in proper position on the platform and adjustment in the head rest was made. S was not required to sit with his head held continuously in rigid adjustment; indeed he was instructed to remove his head from the rest immediately following each judgment made. The aim was to have S's head posture just as nearly normal as possible. After the needed instructions were read to him the room light was turned off and S sat in darkness for several minutes while his eyes were becoming adapted.

An adaptation of the psychophysical method of of average error was used. The homolometer light was turned on thus presenting a small point of light to S either below or above the level of his eyes. At his instruction the experimenter raised or lowered the light with slow and uniform movement until S declared he perceived it level with his

eyes. Immediately after each judgment the light was darkened while the experimenter recorded the setting on the record strip and moved the light carriage to a new position. Again the light was presented and the experimenter raised or lowered it as S instructed, until he perceived it as level with his eyes. Six series of 12 judgments each or 72 judgments in all were made at each sitting, requiring an hour or slightly longer. A rest period of approximately two minutes with room illuminated was had between the third and fourth series, while the experimenter changed the record strip.

The usual precautions for avoiding errors were taken. In an order determined by chance the light was presented an equal number of times from each of three positions above the true level and below the true level. At no time did the Ss see the homolometer as it was shielded by a curtain whenever the room was illuminated. Nor did the Ss at any time know the nature or magnitude of their errors. Every known precaution was taken to guard against assistance from any external sources.

In that part of the investigation dealing with the influence of head tilt each S served on three different days making 72 judgments at each sitting. Three degrees of head tilt were used: normal, 5 degrees up and 5 degrees down. A series of 12 readings was taken with the head in one position, followed by a second series with head in altered position, and so on. The series were arranged in counter-balanced manner for the various Ss.

Results.—I. With what accuracy do individuals localize visual objects as in the same horizontal plane with the eyes, when the usual external cues are excluded? As the best measure of ability in this function the most probable locus of judgments was calculated. That is, the mean of the errors was found, taking into account the algebraic signs—an error above the true level being designated as positive and an error below as negative. This most probable locus was termed the average perceived level. An examination of the data presented in Tables 1, 2 and 3 reveals four significant facts.

(1) Individual differences with respect to the accuracy of localization are great. This is indicated by the extremely wide range over which the average perceived levels fluctuate, extending from 3.73 degrees above the true level for Mr. A to 17.42 degrees below the true level for Miss Q. The true level is approximated by one S only, Mr. B, whose average perceived level is −0.41 (P.E. 0.13) degrees; 58 percent of his judgments fall below the true level. All other Ss definitely miss the true level.

(2) The tendency to make the constant error of dropping the perceived level below the true level is very obvious. The average perceived level for the 17 Ss is 4.87 (S.D., 2.44) degrees below the true level. Only 2 of the 17 Ss give an average

TABLE 1

SHOWING THE MOST PROBABLE LOCUS OF THE PERCEIVED LEVEL FOR EACH SUBJECT

Subjects	Number of Judgments	Average Perceived Level	S.D.	Percent of judgments Above True Level
Mr. A	72	+ 3.73°	1.67°	97.2
Mr. B	72	− 0.41	1.67	41.6
Miss C	72	− 2.45	1.88	11.1
Mr. D	72	− 2.84	1.71	6.9
Mr. E	72	− 4.48	1.40	0.0
Mr. F	72	− 4.65	2.96	0.0
Miss G	72	− 5.66	2.10	2.8
Mr. H	72	− 6.47	2.30	0.0
Mr. I	144	− 7.46	2.04	0.0
Miss J	216	− 4.65	2.50	2.7
Mr. K	216	− 6.27	2.04	0.0
Miss L	432	+ 2.98	3.74	78.0
Mr. M	432	− 4.77	3.56	9.5
Mr. N	432	− 4.83	3.27	6.2
Miss O	432	− 7.46	2.04	0.0
Mr. P	432	− 9.71	3.60	0.0
Miss Q	432	−17.42	3.01	0.0
Av. for all subjects	220.2	− 4.87	2.44	15.1

TABLE 2

SHOWING THE MOST PROBABLE LOCUS OF THE PERCEIVED LEVEL WHEN DOWNWARD MOVEMENT OF THE POINT OF LIGHT WAS INVOLVED AND WHEN UPWARD MOVEMENT WAS INVOLVED

Subject	Downward Judgments			Upward Judgments		
	Number	Perceived Level	S.D.	Number	Perceived Level	S.D.
Mr. I	72	− 8.61°	1.51°	72	− 6.32°	1.84°
Miss J	108	− 5.74	2.64	108	− 3.57	1.47
Mr. K	108	− 7.04	1.94	108	− 5.30	1.46
Miss L	216	+ 1.19	3.24	216	+ 4.82	3.28
Mr. M	216	− 6.22	3.32	216	− 3.36	3.19
Mr. N	216	− 6.69	2.53	216	− 3.09	2.94
Miss O	216	− 7.94	2.61	216	− 6.62	1.73
Mr. P	216	−11.56	3.42	216	− 7.86	2.72
Miss Q	216	−18.44	2.34	216	−16.40	3.25
Average	176	− 7.89	2.62	176	− 5.30	2.43

TABLE 3

SHOWING THE AVERAGE PERCEIVED LEVEL AT THREE DIFFERENT STAGES DURING A SITTING

Subject	Series 1 and 4	Series 2 and 5	Series 3 and 6
Mr. A	+ 4.5°	+ 3.3°	+ 3.4°
Mr. B	− 0.8	− 0.7	− 0.1
Miss C	− 2.1	− 2.7	− 3.0
Mr. D	− 2.5	− 3.2	− 3.0
Mr. E	− 3.8	− 4.6	− 5.0
Mr. F	− 4.1	− 4.4	− 5.4
Miss G	− 4.4	− 5.9	− 6.8
Mr. H	− 6.9	− 6.3	− 5.1
Mr. I	− 6.8	− 6.7	− 6.6
Miss J	− 4.2	− 5.2	− 5.0
Mr. K	− 4.8	− 6.2	− 6.8
Miss L	+ 2.4	+ 1.9	+ 0.1
Mr. M	+ 0.7	− 0.6	− 1.4
Mr. N	− 4.1	− 5.3	− 6.6
Miss O	− 4.1	− 4.9	− 6.9
Mr. P	− 7.5	− 7.4	− 7.8
Miss Q	−12.5	−14.4	−14.6
Average	− 3.59	− 4.31	− 4.75

perceived level above the true level, while the other 15 give average perceived levels below the true level. In fact 8 of these Ss never in a single judgment set the light above the true level. Of all judgments made by the entire 17 Ss only 15.1 percent fall above the true level.

(3) The amount of error seems to be a function of the direction in which the point of light moves. This is clearly indicated by an inspection of the data found in Table 2. For the 9 Ss who served at two or more sittings the average perceived level was calculated separately for the 'downward' judgments and the 'upward' judgments. When the starting position of the light was above the true level, hence necessitating a lowering of the light, the judgment was termed 'downward'; if a raising of the light was required the judgments was termed 'upward.' For every S the average perceived level is lower on the downward judgments than on the upward judgments; and for all Ss excepting Miss L this means that the error is greater on the downward judgments. For

Miss L alone the greatest error occurred on the upward judgments and she alone has an average perceived level falling above the true level. For Miss O, who exhibited the least difference with respect to downward and upward judgments, a difference of 1.32 degrees is found; for Mr. P, who exhibited the greatest difference, it amounts to 3.70 degrees. The average for all downward judgments is 7.89 degrees below the true level and for all upward judgments it is 5.30 below, making an actual difference of 2.59 degrees, which is sufficiently large to indicate that it is not accidental.

(4) There is a tendency for Ss to drop the average perceived level lower and lower at any given sitting, the farther in time they are removed from a view of their surroundings. It will be recalled that the work of a given sitting was divided into six series of twelve judgments each, with a brief rest between the third and fourth series at which time the room was illuminated. In Table 3 the average perceived levels are recorded for the combined first and fourth, second and fifth, third and sixth series, thus showing the average perceived level at three different stages of time with respect to S's view of the room surroundings. Examination of these data shows that 10 of the 17 Ss are consistent in dropping the average perceived level successively lower at the second and third stage; 13 of the 17 Ss drop it lower at the second stage; 3 only, show a higher average perceived level at both the second and third stage. The means for the average perceived level for all Ss are 3.59, 4.31 and 4.75 degrees below the true level for the three successive stages.

II. Does practice in this function without knowledge of results bring improvement in the function?

Within the limits of the present experiment the evidence shows that practice does not result in improvement. Two of the Ss practiced for three days and six others practiced for six days. The average gross error of judgment for each day was calculated; the data are presented in table 4. Any improvement in ability should of course result in decreased errors. An inspection of the data discloses that for no S is any consistent improvement found; rather the tendency is to grow

TABLE 4

SHOWING AVERAGE GROSS ERRORS (ALGEBRAIC SIGNS NOT CONSIDERED) FOR EACH DAY FOR ALL SUBJECTS HAVING THREE OR MORE SITTINGS

Subject	1st day	2d day	3d day	4th day	5th day	6th day
Miss J.....	4.55°	4.03°	5.43°			
Mr. K.....	5.95	6.85	6.33			
Miss L.....	2.98	3.33	3.77	5.53°	3.48°	5.60°
Mr. M.....	1.45	3.58	5.07	5.57	6.40	8.30
Mr. N.....	5.32	8.21	4.32	4.22	4.13	4.18
Miss O.....	4.97	7.45	7.50	7.78	7.90	8.13
Mr. P.....	7.61	9.10	7.89	8.28	11.54	13.86
Miss Q.....	13.89	17.04	18.28	18.26	18.20	18.83
Average.....	5.84	7.45	7.32	8.27	8.61	9.81

decidedly worse after the first day. If a composite practice curve were plotted using the daily mean of the average gross errors as the measure of attainment, *i.e.*, 5.84, 7.45, 7.32, 8.27, 8.61, 9.81 degrees, the resulting curve would in no wise resemble a learning curve. It is possible that beneficial results might have been found if practice had continued longer, though the experimenter has no reason for believing such to be the case.

III. Does the tilt at which the head is held materially alter the accuracy of localization?

A summary of the data bearing on this question is presented in Table 5. Study of these data shows that with the head tilted 5 degrees above normal posture the average perceived level is slightly raised for 9 Ss, slightly lowered for 6 Ss, and remains unchanged for 1 S. With the head tilted 5 degrees below the normal posture the average perceived level is slightly raised for 8 Ss and slightly lowered for 8 Ss. No consistent trend is found in either the effect of upward tilting or downward tilting. In the case of the first 12 listed Ss the differences are so slight that it is evident they are not statistically reliable. However, in the case of the last 4 Ss the difference found may be genuine. The $P.E._{s(diff.)}$ when calculated for each are as follows: Miss M2, $P.E._{(diff.)} = .245$; Mr. N2, $P.E._{(diff.)} = .471$; Miss O2, $P.E._{(diff.)} = .303$; Mr. P2, $P.E._{(diff.)} = .253$. Interpreted in terms of chance this means

TABLE 5

SHOWING THE MOST PROBABLE LOCUS OF THE PERCEIVED LEVEL FOR EACH SUBJECT WITH HEAD IN THREE DIFFERENT POSITIONS

Subject	Head in normal position		Head tilted 5 degrees up		Head tilted 5 degrees down		Greatest Difference
	Perceived Level	S.D.	Perceived Level	S.D.	Perceived Level	S.D.	
Mr. A2....	− 2.31°	2.36°	− 2.12°	2.28°	− 2.33°	2.36°	.21°
Mr. B2....	− 3.66	4.08	− 3.27	4.02	− 3.47	2.70	.39
Mr. C2....	− 6.66	2.51	− 6.86	2.87	− 6.85	2.54	.20
Mr. D2....	− 7.65	1.82	− 7.48	2.02	− 7.20	2.33	.45
Mr. E2....	− 7.93	2.75	− 7.77	3.03	− 7.70	2.55	.23
Miss F2....	− 9.45	3.80	− 9.48	3.52	− 9.36	3.70	.12
Miss G2...	−10.13	3.05	− 9.30	3.33	− 9.94	2.75	.83
Mr. H2....	−10.45	3.32	−10.63	3.33	−10.54	3.05	.18
Mr. I2.....	−10.90	3.82	−10.76	3.75	−10.95	4.08	.19
Miss J2....	−11.51	3.44	−11.80	3.86	−11.08	3.56	.72
Miss K2...	−13.84	2.97	−13.65	3.20	−14.18	2.66	.53
Mr. L2....	−15.44	1.94	−15.44	2.10	−15.63	1.87	.19
Miss M2...	− 4.79	2.30	− 5.45	2.18	− 4.10	2.20	1.35
Mr. N2....	− 7.27	4.07	− 7.87	4.08	− 6.55	4.31	1.32
Miss O2...	−10.09	2.60	− 8.77	2.93	−10.16	2.49	1.39
Mr. P2....	−14.57	2.25	−13.50	2.63	−14.75	1.80	1.25
Average....	− 9.16	2.94	− 9.01	3.07	− 9.05	2.81	

that for Mr. N2 the chances are more than 97 out of 100 that the difference is greater than 0; for the other three the chances are better than 100 out of 100 that the difference is greater than 0. If the means of the average perceived level for all Ss be considered, *i.e.*, 9.16, 9.01, and 9.05 degrees respectively, the difference between the three head positions is of course insignificant.

Though the evidence is not wholly unequivocal it seems fair to conclude that changing the angle of tilt of one's head as much as 5 degrees above or below normal posture does not materially alter the accuracy of localization.

DISCUSSION OF RESULTS

Several items presented above call for a brief discussion. How shall we account for the almost universal tendency to drop the perceived level below the true level? In other words

why do subjects so universally err by localizing a visual object as being in the horizontal plane of the eyes when in point of fact it may be as much as 15 or 20 degrees below that plane? The present writer would give much the same explanation as Hoppeler (1) gave as the most reasonable one. For the most part one's eyes are in service or at rest with the line of vision considerably below the horizontal plane. When all other cues save the kinesthetic of the ocular muscles are excluded the eyes tend to seek their more accustomed position in which the line of vision is below the horizontal and yet it is interpreted as horizontal. This explanation might be experimentally tested by tilting the S's head upward at an angle approximating the angle at which the normal line of vision falls below the horizontal.

It was found that a majority of the Ss exhibited a tendency to drop the average perceived level lower and lower during any given sitting the further in time they were removed from a view of their surroundings. This fact seems in some measure to support the theory of Koffka (2) concerning spacial localization. He argues that localization is made by virtue of 'points of anchorage,' which of course are lost in the dark room resulting in a loss of ability to localize accurately. In this present investigation such a frame of reference was not lost instantly when the lights were turned off, but persisted for a time. This would account for the more accurate localization in the early stage; as the frame of reference was gradually lost the errors of perception became larger and larger. The findings of this present study would then in this respect harmonize with Koffka's point of view.

Again, it was found that practice without knowledge of results failed to bring any improvement in the function. This finding is in keeping with the position of Thorndike (3) and others that mere repetition is not sufficient to produce learning. It would seem that this function, *i.e.*, judging when a point of light is in the same horizontal plane with the eyes has several features making it adaptable for studies dealing with the influence of guidance in learning.

It remains to say something by way of explanation for the

discrepancy in the means of the average perceived level for the two groups of subjects with which the work was done. This mean for the 17 Ss as reported in Table 1 is 4.87 degrees below the true level, while that for the 16 Ss as reported in table 5 (normal head position) is 9.16 degrees below the true level. Several factors may have played a part. When some of the earlier Ss were serving, the carriage of the homolometer made a slight noise as it moved up and down. It is possible that in some way the Ss were assisted by this noise and consequently did not make such large errors. Also as pointed out in the discussion concerning practice Ss tended to make the least errors during the first sitting. The earlier group of Ss contains many who served for one sitting only, and as the means are computed their results weighed just as heavy as the others. This tends to decrease the apparent errors as contrasted with the second group where all Ss served for three sittings. Once again, for some inexplainable reason there seems to be a difference between the two groups, as indicated by the fact that the first group contains two persons whose average perceived level is decidedly above the true level, as well as one that is extremely low. Though such a discrepancy mars the perfection of an investigation, the experimenter is positive that in this case the conclusions are in no wise invalidated.

Conclusions

The results of this investigation may be briefly summarized as follows:

I. Concerning the accuracy with which individuals localize visual objects as in the same horizontal plane with their eyes, when the usual external cues are excluded.

a. Individual differences are great, the average perceived levels ranging from 3.73 degrees above the true level to 17.42 degrees below.

b. The constant error of dropping the average perceived level below the true level is obvious.

c. The amount of error seems to be a function of the direction of movement of the visual object.

d. Subjects tend to drop the average perceived level

progressively lower as they are removed in time from a view of the room surroundings.

II. Concerning improvement in the function with practice.

Practice, continuing at least six hours, does not result in improvement.

III. Concerning accuracy as altered by the tilt of head.

Accuracy is not materially altered by changing the tilt of the head as much as 5 degrees above or below the normal posture.

(Manuscript received August 31, 1933)

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NEW OBSERVATIONS RELATED TO THE PROBLEM OF COLOR-CONTRAST

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The following phenomenon is undoubtedly related to the problem of color-contrast. When a disk such as the one illustrated in Fig. 1 is rotated about 10 times per second before

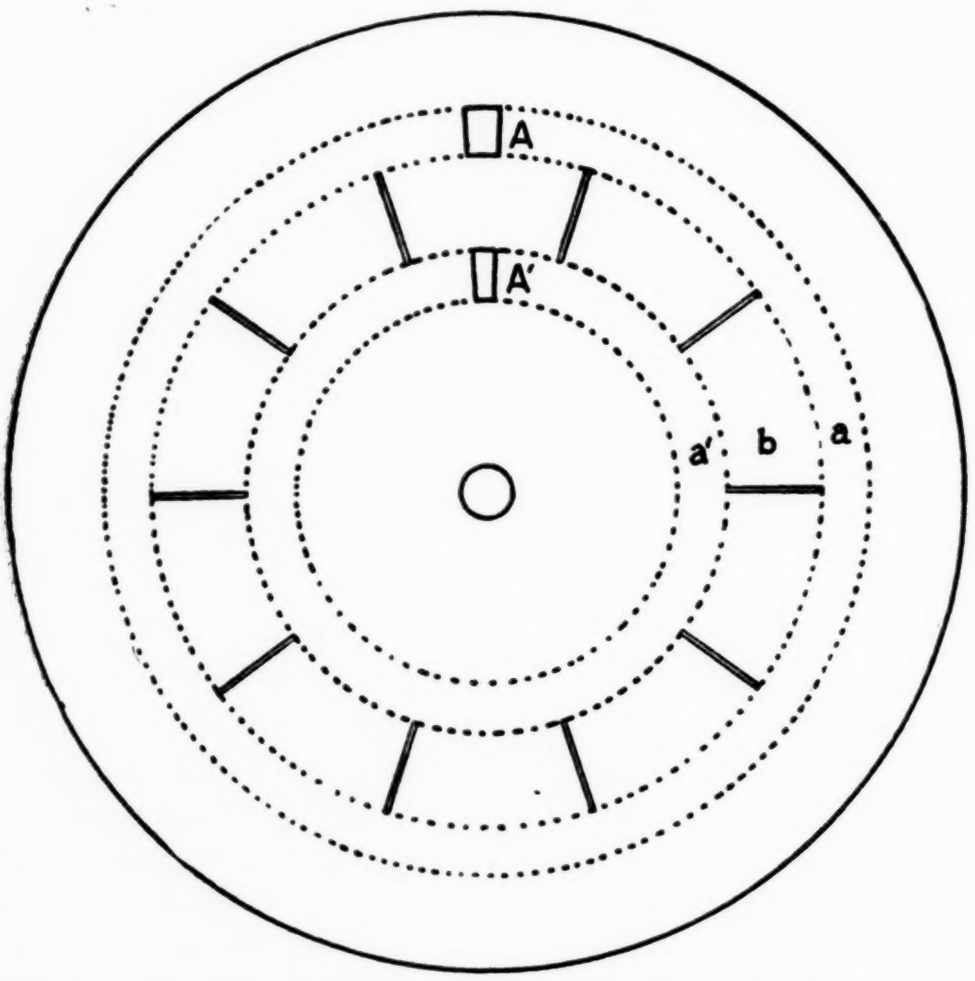

FIG. 1. The disk is made of black cardboard. *A* and *A'* are open slits 5° wide. Each of the ten slits in zone *b* is ½° wide. Zones *a* and *a'* are ½ in wide and zone *b* is 1 in wide.

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a bright white surface (402.7 c. per sq. ft.) and observed at a distance of 12 ft, zones *a* and *a'* appear blue and *b* yellow. For the sake of convenience the symbols, α, β, and α', will be used to refer to the three zones of the retina corresponding to the three zones of the disk, *a*, *b*, and *a'*, respectively. The yellow color of zone *b* must be regarded as a color-contrast effect induced by intermittent stimulation of α and α', for it cannot arise in any other way. The frequency of stimulation for β is so rapid (100 per second) that it is equivalent to steady stimulation. Hence the yellow color cannot be attributed to the intermittence of stimulation of zone β. Furthermore if α and α' are left unstimulated zone *b* is seen white. This fact indicates that intermittent stimulation of α and α' is an indispensable condition for the production of the yellow of zone *b*.

β, in turn, aids in the production of the blue color of *a* and *a'*, for the blue cannot be produced if β is left unstimulated, for under these conditions *a* and *a'*, are seen flickering white.

Fig. 2 illustrates a possible conception of what takes place

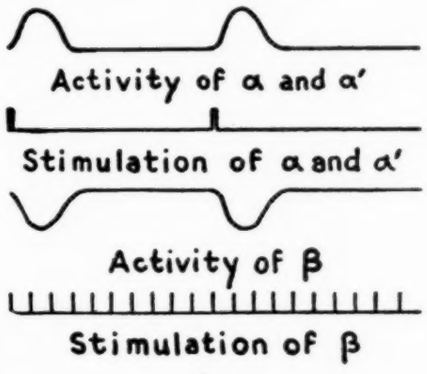

FIG. 2

in the three zones of the retina corresponding to the three zones of the disk. The activity of zone β depresses the activity of α and α' during the periods between successive stimulations and thus helps to modulate it. And it may be supposed that this modulation subserves the production of the blue sensation. At least it is in accordance with Troland's [1]

[1] L. T. Troland, The enigma of color vision, *American Journal of Physiological Optics*, 1921, **2**, 43.

In a previous paper (G. A. Fry, Modulation of the optic nerve current as a basis for color-vision, *American Journal of Psychology*, 1933, **45**, 488–92) the writer elaborated Troland's theory and attempted to show that it is adequate to account for the color-sensations produced with intermittent white light.

suggestion, namely, that the transmission of color impressions from the retina to the cortex might depend upon the modulation of the currents of impulses in the various optic nerve fibers. Retinal zone β acts as if it were steadily stimulated and shows no tendency to set up a rhythmic modulation of its own. The modulation which it acquires, and which subserves the yellow sensation, must be induced upon it by the rhythmic activity of α and α'. This could be brought about if the activity of β tended to wax and wane reciprocally with that of α and α', being depressed when the activity of α and α' waxes and enhanced when it wanes, but one encounters difficulty if he tries to conceive what mechanism underlies this reciprocal waxing and waning.

A puzzling fact is that varying the phase of slit A with respect to slit A' has no effect upon the colors of the various zones. If this fact is to be accounted for in accordance with the above explanation, it must be assumed that when A and A' are out of phase with each other, part of the elements of retinal zone β function reciprocally with zone α and part with zone α', and that these two parts function out of phase with each other. It is doubtful whether elements within the same area can function out of phase with each other,[2] *i.e.*, in just any haphazard phase relation, and for this reason some doubt must affix itself to the above explanation of the color-contrast effect.

Furthermore the above explanation requires that the modulation rhythms in all the fibers leading from retinal zone α, and likewise from retinal zone α', be synchronized. This synchronization is brought about by intermittent stimulation in the case of the disk in Fig. 1, but the case would be different if α and α' were steadily stimulated with blue light. Assuming that stimulation of a given area of the retina with blue light modulates the activity of the elements of that area, it is necessary to postulate some additional mechanism for synchronizing the rhythms of the various elements. In this respect it would be satisfactory to postulate a special exciting-

[2] Since retino-cortical paths interact with each other at synapses, the activity in one path might interfere with the modulation rhythms in neighboring paths, unless the rhythms in the various paths were synchronized.

substance in the retina which is independent of the rods and cones, and whose excitation comes prior in the visual process to the excitation of the rods and cones. The response of this exciting substance could consist in a wave-like disturbance which involves the whole of the retinal area stimulated. On the other hand the rods and cones might contain the primary exciting-substance and they might react to light independently of each other, but yet interaction at the synapses could conceivably synchronize the modulation rhythms. However, it may be that interaction at the synapses is such that the activity in one retino-cortical path would interfere with modulation patterns in other paths unless the modulation rhythms in the various paths were synchronized to start with.

The principles outlined above may be extended to apply to any instance in which a contrast-color is induced upon a neutral field. But this extension requires several assumptions. (1) The different qualities of hue must have unique forms of modulation waves. (2) Since the induced color is complementary to the inducing color, modulation waves corresponding to complementary colors must be complementary in every case, for the activity of the responding field must wax and wane reciprocally with that of the inducing field. (3) Just as the different qualities of hue in the color-circle form a gradual transition from one to the other, so the corresponding modulation wave-forms must also form a continuous transition. One can conceive of many systems of wave-forms which will meet the above requirements. One such system is illustrated in Fig. 3.

It is necessary to distinguish between two aspects of the problem of color-contrast, (1) the induction of a color upon the responding field, and (2) the formation of a border between the inducing and the responding fields. So far we have dealt only with the first aspect of the problem; let us turn now to the border aspect.

In certain cases the exciting and responding fields are blurred into each other, but phenomenally a sharp border is obtained. The disk in Fig. 4 is so constructed that when it is rotated rapidly the effect upon the retina is substantially the

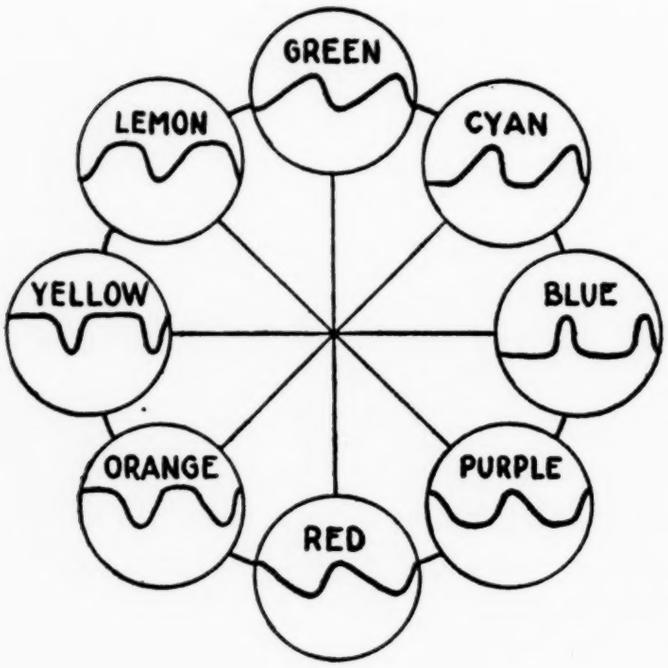

FIG. 3

same as if the stimulus consisted of a blurred gray band upon a blue disk. But instead of looking blurred the yellowish-gray band is seen sharply defined. The position of the phenomenal borders are indicated by the dotted lines. These lines cross the ogive curves at their mid-points. In this case

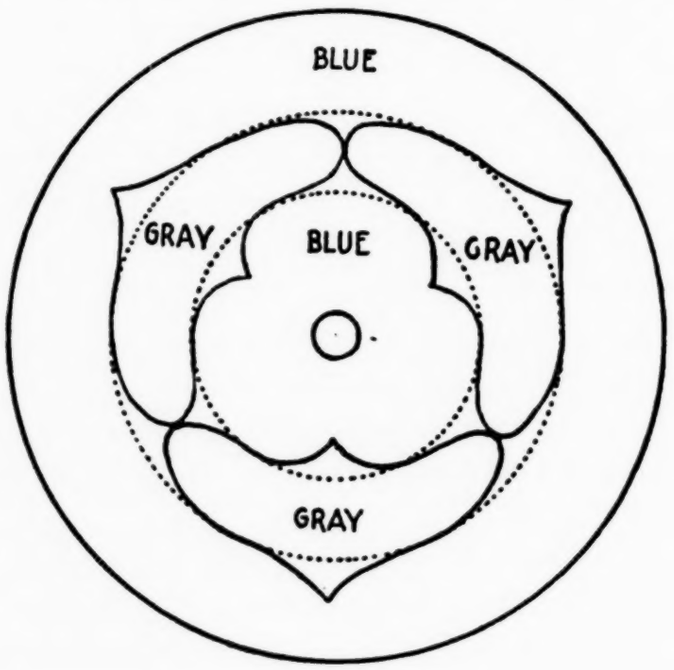

FIG. 4

color-contrast involves the creation of a border where no border exists in the physical stimulus.

The following phenomenon is related to the one just described. When a disk like that in Fig. 5, 8 in. in diameter, is

FIG. 5

rotated anticlockwise 15 times per sec., illuminated with a 250 watt lamp placed 2 ft away, and observed at a distance of 12 ft, a yellow band is seen between two smaller blue bands as indicated by the dotted lines. The borders between the yellow and the blue bands are seen sharply defined. These borders have no stimulus correlates inasmuch as they cross the edges of the white sectors at an angle.

The above experiment may be varied in the following way. Three disks may be designed, the first of which comprises the inner two zones of the disk in Fig. 5, the second the three inner zones, and the third all five zones. By placing the first disk on top of the second, and the second on top of the third, a pattern like Fig. 5 can be obtained, and like it gives a yellow band between two blue bands when observed under similar conditions. The three disks can be shifted in any angular position with respect to each other, but no matter what arrangement is used, the same phenomena are obtained. This

fact indicates that the three groups of retino-cortical paths corresponding to the three bands function as units in some way, but it is hard to picture the precise nature of the processes involved.

Summary

If a given area of the retina is steadily stimulated with white light, a yellow contrast color may be induced upon it by stimulating the surrounding area intermittently with white light, the same as if the surrounding area were steadily stimulated with blue light.

A theory of color-contrast is proposed which can account for the facts and which is in harmony with Troland's suggestion that the transmission of color impressions from the retina to the cortex depends upon the modulation of the optic nerve current.

It is shown that under certain conditions the border between the exciting and responding fields may be blurred but yet phenomenally a sharp border is obtained. The physiological processes involved in the formation of this phenomenal border are not clearly understood.

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CHANGES OF VISUAL ACUITY IN ONE EYE UNDER THE INFLUENCE OF THE ILLUMINATION OF THE OTHER OR OF ACOUSTIC STIMULI

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Visual acuity is characterized by the minimum interval still perceptible to the eye between two objects (points, lines, squares etc.—*minimum separabile*). A decrease of the interval indicates an increase in visual acuity and vice versa. Still, the border line between black and white (or rather to say, between light and dark) is never sharply delineated upon the retina, owing to the imperfect accomodation of the eye and the phenomena of spheric and chromatic aberration; the region of maximum illumination is never directly contiguous with that of minimum illumination, but there is a gradual melting of one into the other. This is the cause of the well-known phenomena of irradiation of bright surfaces.

Thus, if we have two squares *A* and *B*, black on a white ground, the distribution of illumination upon the retina, corresponding to the interval between the squares, and consequently the excitation in the visual center, will be represented by the curve *bcd*, Fig. 1*a*. This interval may or may not be perceptible to the eye and if perceptible it may be minimum or it may exceed this quantity; it all depends upon two facts: 1. the level of the ordinate *bcd* (corresponding to the actual illumination of the retina, sive exitation of the visual center), 2. the level of the ordinate corresponding to the liminal illumination, sive exitation. Let us assume that the ordinate corresponding to the threshold of visual perception, coincides with *km*. In the case if the actual illumination (s. exitation) is represented by the figure *kmm'k'* is minimum, that is no more than sufficient to produce a different degree of exitation in two adjacent elements of the retina, the interval between

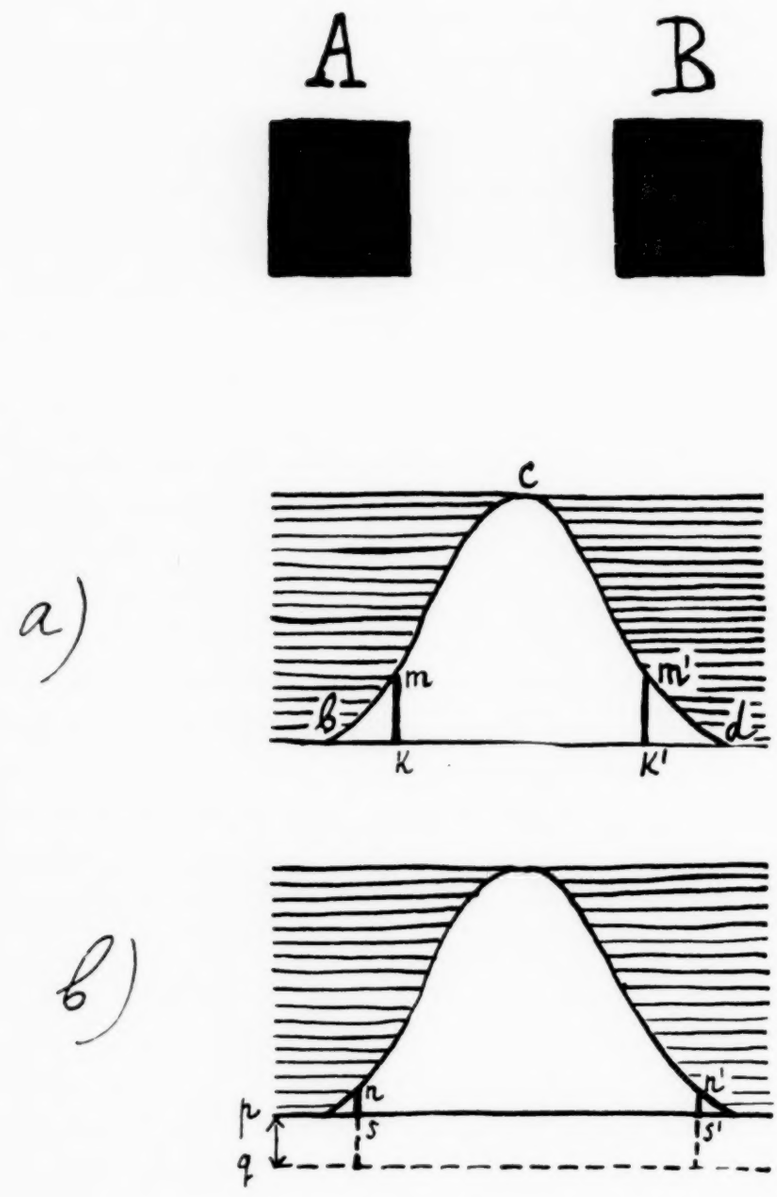

FIG. 1

the squares is also minimum. Once diminished, it becomes no more perceptible. Suppose now that, for some reason or other, our brain center (the receptor of all visual stimula) is in a state of subliminal exitation characterized by the ordinate *pq* (Fig. 1*b*). In this case the threshold ordinates are shifted from *km* and *k'm'* to *sn* and *s'n'*. In other words the minimum limits are exceeded; the interval between the squares induces an above-threshold exitation. To make it minimum again, that is fit only to excite one separate receptive element of the

retina, we should diminish the distance between the squares *A* and *B*. The meaning of it all is that in the case of black objects upon white ground, an additional excitation of the brain center produces an increase in visual acuity. A heightening of the effect of the irradiation of the white surfaces is what underlies this phenomena.

Now if white squares on a black ground are presented and consequently the object to be discerned is not a white interval between two black surfaces, but a black interval between two white ones, the situation is naturally reversed. Under the influence of an additional excitation of the brain the effect of irradiation of the white surfaces is increased. The result is that the black interval between the white surfaces is partly obliterated and may even become totally invisible. To make it perceptible again we should augment the distance between the squares. In other words, in the case of white objects upon a black ground every additional excitation of the brain calls forth a decrease of visual acuity.

An additional excitation of the brain can be produced by different indirect stimuli, such as an illumination of the other eye, a tone, an odor. In the latter cases the excitation of the neurons directly involved is transmitted to the neurons of the optical nerve, thanks to their anatomical proximity; and thus a subliminal excitation is created in the visual center.

These theoretical conjectures were fully corroborated by a series of our experiments, a part of which had been published already. I reported in 1930 that in the case of black objects on a white ground an improvement of the visual acuity of one eye occurs, if the other eye is simultaneously illuminated (1); the same if an acoustic stimulus is simultaneously applied (2). In the case of white objects on a black ground, on the contrary a deterioration of visual acuity is effected. It is important to note that the effect of the auxiliary stimulation (facilitating in one case, depressing in the other) is of some duration; it lasts for several minutes even after the stimulus had been removed. Fig. 2 shows the gradual extinction of the stimulation effect, a phenomenon observed by us in a number of persons.

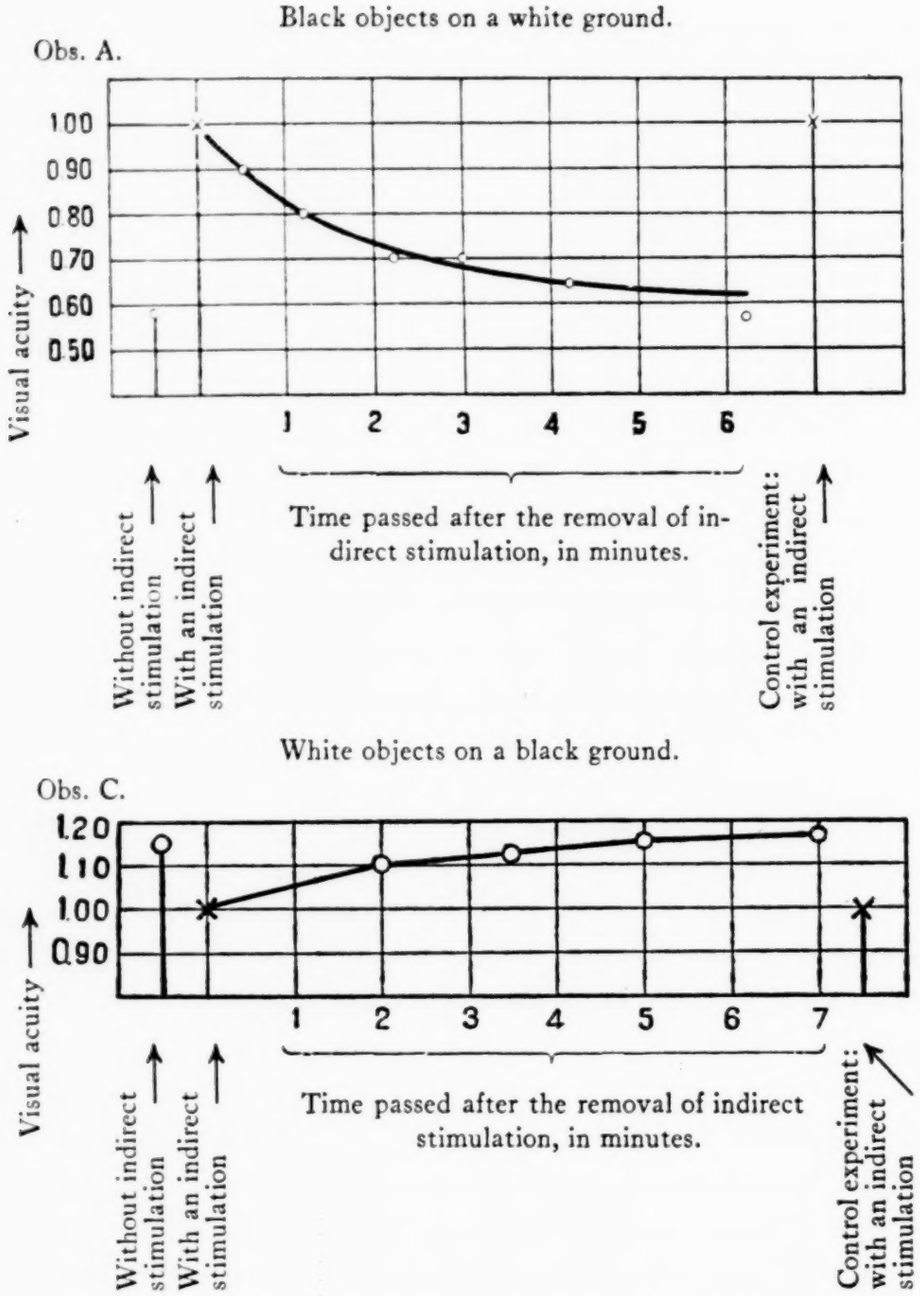

FIG. 2. Gradual extinction of the indirect stimulation (illumination of the other eye) effect upon visual acuity.

After the above mentioned reports had been published, analoguous experiments demonstrating the influence of the illumination of one eye upon the visual acuity of the other, both for black-on-white and white-on-black configurations were repeated by my collaborator Mrs. O. P. Holmskaya. The results obtained were as follows:

Comparative thresholds for visual acuity in one eye in the case of black objects on a white ground; the values are given in angle-seconds.

Observers	Date	Normal		Light	
		Mean	A.D.	Mean	A.D.
M	4.1.31	17	±1.3	13	±1.0
W	28.1.31	41	±1.6	31	±1.5
B	19.1.31	20	±0.8	16	±1.8

The values under 'light' refer to the performance when the left eye is illuminated; the values under 'normal' to the performance when the left eye was free from any stimulation.

Comparative thresholds for visual acuity in one eye in the case of white objects on a black ground.

Observers	Date	Normal		Light	
		Mean	A.D.	Mean	A.D.
M	1.3.31	58	±2.0	67	±5.0
I	9.2.31	25	±3.7	33	±2.6
T	27.2.31	43	±3.7	51	±2.0

Furthermore in 1931 Dr. Gotoh (Japan) undertook a verification of my data (**3**). His results fully corroborated the fact that in the case of black objects on a white ground the visual acuity of the right eye is improved through illumination of the left eye, in the case of white objects on a black ground, on the contrary the visual acuity is deteriorated under the same condition.

The explanation of this fact, as we give it in the present paper, consists in the supposition that indirect stimuli increase the irradiation of white surfaces. It became, therefore, urgent, to ascertain experimentally if indeed the irradiation of white surfaces presented to one eye can be increased through the application of indirect stimuli, such as an illumination of the other eye, an acoustic stimulus, etc. To that end we undertook in 1931–32 a series of experiments which fully confirmed our thesis and established definitely the fact of a

heightening of the irradiation of the white surfaces under the influence of indirect stimulation (**4**).

To sum up: the fact of an opposite action of indirect stimulation in the case of black objects on a white ground and in the case of white objects on a black ground—strange as it may seem at the first glance—can be considered as fully established.

We may still mention the experiments of Wilcox (**5**), which in full accordance with the theory exposed, point to a different interrelation between visual acuity and the degree of illumination of the object in discriminating black objects on a white ground or white on black. In the first case (black on white) visual acuity continuously increases with the increase of illumination; in the second (white on black) it soon reaches a maximum corresponding to a comparatively low degree of illumination; with a further increase of illumination visual acuity suffers a rapid deterioration.

Quite recently two reports by G. W. Hartmann (**6**, **7**) have been published in the Journal of Experimental Psychology, both treating the question of the changes in visual acuity under the influence of indirect stimulation. The purpose of G. W. Hartmann was to repeat and check our experiments; yet he comes to a different conclusion; so he declares: "in opposition to an earlier experiment by Kravkov our study indicates an increase in visual acuity of the right eye for *both* black objects on a white ground and white objects on a black field when the left retina is simultaneously strongly illuminated . . . tones . . . , odors . . . , tactile and . . . painful stimuli all enhance the discrimination of black-on-white or white-on-black configurations." At the same time he advances the opinion that "the greater energy of the brain process caused indirectly by stimulation of the 'unused eye' has the same effect as direct local increase in the illumination of the testing squares." Hereby he falls into contradiction not only with my own experiments and those of Gotoh but also with the above mentioned investigation of Wilcox, the latter proving that an increase in the illumination of the testing squares effects at a certain point a deterioration in visual acuity, if

white objects on a black ground are presented; while in the case of a black-on-white configuration visual acuity continuously increases.

A close examination of Hartmann's methods of experimenting and of the figure-tables upon which his conclusions are based, permits us, however to call his inferences into question.

The chief mistake lies, to our opinion, in the alternating of readings taken under noraml conditions (that is without indirect stimulation) with those taken while indirect stimulation was being applied, the pause between both generally not exceeding one minute. Thus readings 1 and 2 were 'normal,' reading 2 and 3—'with an indirect stimulation,' reading 5 and 6 'normal' and so on. It is quite clear that in this way the effect of the indrect stimulation never had time to fade out entirely (the time required for its complete extinction being equal to several minutes, a fact established by our experiments, as well as by those of Gotoh, see the curves on Fig. 2); his 'normal' readings could not be considered as really 'normal'; the central excitation produced by the indirect stimulation still lasted at the moment they were taken.

The conditions of our experiments were different; we took first a series of readings without indirect stimulation, then a series with a simultanuous application of an indirect stimulus. This difference of methods accounts for the difference of results.

As we say, in Hartmann's experiments the indirect stimulation effect renewed every two minutes, was still manifest at the moment when 'normal' readings were taken. Consequently, all his readings, when analyzed, appear as readings taken under stimulation. It is then to be expected that the average values under both readings ('normal' and 'light') would not offer any palpable difference. So it is fact. Nearly all the average differences between the means under both headings lie within the limits of the average deviations. (See the figure-tables in Hartmann's reports.)

Still, there remains a small difference between the means under both headings, though insignificant from the point of

view of statistics (namely 0.16, while A.D. equals to 0.32, 0.37, 0.23 and 0.26).

This difference is most probably accounted for by the fact that the two initial readings 1 and 2 (which we suppose to be more rough than the following—this being the rule in every observation) always fall under the heading 'normal' and thus influence the means of this group, tending to make its total a little higher that that under the heading 'with indirect stimulation.'

On the ground of all these reasonings we come to the conclusion that neither our experiments (together with those of Gotoh and Wilcox), nor the theory expounded in the beginning of the present paper, are disproved by the experiments of G. W. Hartmann.

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THE FACILITATING EFFECT OF STRONG GENERAL ILLUMINATION UPON THE DISCRIMINATION OF PITCH AND INTENSITY DIFFERENCES

BY GEORGE W. HARTMANN

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I. Earlier Evidence of the Influence of Visual Stimuli upon Auditory Functions

In the year 1669, the distinguished Copenhagen anatomist Thomasius Bartholinus [1] announced that partially deaf individuals could hear better in the light than in the dark. This observation remained unchecked until 1796 when the medical faculty of the University of Göttingen awarded its prize for the best dissertation dealing with the effects of light upon the human constitution to Johann Christoph Ebermaier [2] and Ernst Horn.[3] Ebermaier was especially insistent upon the beneficial influence of light in cases involving diseases of the ear, and recommended the concentration of light rays into the auditory tract by means of a convex lens.

A less obscure and more accessible writer on this theme is Victor Urbantschitsch, a pioneering Viennese dabbler in many odd fields and an investigator worthy of more attention than he has received.[4] In a paper full of many strange observations,[5] he reported that one's auditory acuity fluctuated with the light or darkness, being greater under bright conditions.

[1] *De luce hominum et bruttorum*, Hafniae, 1669, p. 250.

[2] The original Latin volume appeared at Göttingen in 1797; a German version with the title, "*Versuch einer Geschichte des Lichts in Rücksicht seines Einflusses auf die gesamte Natur und auf den menschlichen Körper, ausser dem Gesichte, besonders* "was issued at Osnabrück in 1799.

[3] *Ueber die Wirkungen des Lichtes auf den lebenden menschlichen Körper mit Ausnahme des Sehens*, Königsberg, 1799.

[4] He is mentioned but once in Boring's *History of Experimental Psychology* and that in connection with the complication experiment.

[5] Ueber den Einfluss einer Sinneserregung auf die übrigen Sinnesempfindungen, *Pflügers Archiv.*, 1888, **42**, 154–182.

The common objection raised by concert hall visitors that they close their eyes when following an intricate musical rendition he explained by the fact that this closure probably blocks the distraction of attention through visual objects in the environment. If, however, we place ourselves in the position of an eavesdropper, an involuntary elevation of the eyelid results, even when the source of the sound is not accessible.[6] Urbantschitsch also noticed that subjective ear noises were diminished by shading or closing the eyes, whereas light led to an increase in the buzzing.

Until very recently, this fact has been neglected by psychologists, and either dismissed as a scientific curio or forgotten because it failed to fit into any systematic theory of mental life. As an indirect outgrowth of the Gestalt viewpoint, a respectable body of information on the inter-relations of the senses [7] has been accumulated within the last few years, all of which support the view that some functional unity characterizes the entire sensorium; in other words, what we see is literally influenced by what we are hearing, touching, smelling, etc., at the moment. Presumably, the converse relations also hold. If one may hazard a generalization, it seems that no receptor operates in isolation, but that the perceptions primarily mediated by a single modality are supplemented and modified by stimuli simultaneously affecting all the other sense organs.

The specific influence of light stimulation has lately been studied by two Viennese physicians—Freund, an actinologist, and Hofmann, an otologist.[8] Acquainted with Ebermaier's results but profoundly skeptical of them (since it seemed im-

[6] A number of famous paintings of historic assassinations display this reaction clearly. The phenomenon received its classical description in Darwin's *Expression of the Emotions in Animals and Men*, p. 281 (New York, 1890). He writes: "The habit of raising the eyebrows having once been gained in order to see as quickly as possible all around us, the movement would follow from the force of association whenever astonishment was felt from any cause, even from a sudden sound or idea."

[7] The most accessible summary of this topic is probably to be found in an article by the writer on Changes in visual acuity through simultaneous stimulation of other sense organs, *J. Exper. Psychol.*, 1933, **16**, 393–407.

[8] *Cf.* Licht und Hören, *Medizinische Klinik*, 1929, 1–5, and Ueber den Einfluss des Lichtes auf die Funktionen des Gehör-und Geruchsorgans, *Strahlentherapie*, 1929, **34**, 110–116.

probable that the transverse light waves of the ether could affect the longitudinal sound waves), they nevertheless found that 50 percent of their clinical cases confirmed the statement of the eighteenth-century author. They radiated for five minutes with a thousand-watt lamp the deafer side of the heads of patients suffering from high-grade afflictions of the inner ear, and using the whisper test they found an average improvement equal to double the customary acuity. With long wave-lengths, the gain set in immediately upon illumination, remained at a fixed level for a time, and then rapidly declined; with short wave lengths, however, little alteration would be detected during the radiation, the improvement occurring only after a latent time when reddening of the skin set in. In general, the greater the intensity of the lamp, the better the results.

While Freund and Hofman are plainly aware that these intersensory phenomena must have a 'central' neurological explanation, they do not distinguish sufficiently between the direct local effects of the light rays upon the skin and auditory tract, and the indirect remote influences traceable to optical stimulation. From the psychologist's standpoint, the latter relationship is the more significant. Otologists have long noted that the auditory perception of hard-of-hearing patients fluctuates with the weather—it tends to be better on bright warm days, and poorer on raw or clouded days. The Dane Bartholinus (*supra*) had reported improved hearing even by the dim light of a candle. More recently, the Moscow biophysicist, Lazareff, has popularized the following simple demonstration: Strike a piano key with the pedal depressed and while the tone lasts, silently switch an electric light bulb on and off; one hears in the same rhythm the swelling and fading of the tonal intensity! If the eyes are closed or covered, the effect fails to occur.

Using a modified Zwaardemaker olfactometer, Freund observed that his subjects were able to detect the odoriferous bodies at a much greater distance after exposure to strong illumination—in one case the identification was consistently correct at seven times the 'normal' distance. The facilitating

effect was of relatively short duration—two to five minutes—but undoubtedly of 'central' rather than local origin, since the improvement practically disappeared when the subject was blindfolded and his nasal cavities radiated. Certain familiar phenomena lend plausibility to this finding—*e.g.*, the well-known fact that habitual smokers do not enjoy the act in the dark, that blind men rarely smoke, that those who can identify various brands of tobacco by the smell lose this capacity in the dark, that even professional wine-tasters cannot distinguish between red and dark wine in the dark,[9] and that the custom of serving luxurious meals under festive lighting conditions appears to have a good physiological foundation.

One possible explanation of Freund's results may be derived from a consideration of the local effects of light as such. That solar radiation or its equivalent produces better circulation, congestive hyperæmia, accelerated metabolism, and subsequent benefits to all cell and tissue functions has been demonstrated by numerous exact investigations. The increase in muscular output after ultra-violet radiation is itself traceable to a heightening of general neural functions. But this local theory does not suffice to explain the reciprocal effects of other sense organs upon vision. An adequate generalization concerning intersensory processes must inevitably be founded upon our knowledge of central nervous activity. The possibility of an anatomical connection between the acoustic and optic centers is provided by the proximity of the lateral geniculate body and the anterior quadrigeminal body (the optic centers in the mesencephalon) to the medial geniculate body and the posterior quadrigeminal body, respectively. A reciprocal influence of these two spheres through irradiation or kindred excitation is thus readily conceivable. Similar, but somewhat more complex, linkages are available between the optic and olfactory centers.

Von Schiller and Wolff,[10] in a study evidently executed simultaneously with the one here reported, made a number of

[9] Hans Henning, *Der Geruch*, 1924.

[10] Gegenseitige Beeinflussung der optischen und der akustischen Helligkeit, *Zsch. f. Psychol.*, 1933, **129**, 135–148.

significant qualitative observations pertaining to this new field of intersensory phenomenology. If the subject is seated before a *Ganzfeld*, *i.e.*, a large white screen embracing the entire visual field whose total illumination can be regulated by the experimenter, his acoustic experiences can be affected by his visual under the following conditions:

1. The tone must possess a certain lability, *i.e.*, be of slight intensity and capable of gradual alteration in pitch, but the direction of the change must not be observable by the subject.

2. The optical brightness must be definitely supraliminal.

3. The optical and acoustic 'brightness' must not be very different from each other.

4. A gradual alteration in the brightness of the total visual field induces a brightness change in a simultaneously presented tone, provided they are both moving in *opposed* directions, *i.e.*, an increased (decreased) visual brightness enhances (diminishes) the apparent auditory brightness of a tone which is gradually being lowered (raised) in pitch.

II. The Present Problem: Apparatus and Procedure

In a previous study (see footnote 7, *supra*) the author submitted evidence indicating that a slight but consistent increase in visual acuity occurs whenever the other major sense organs are simultaneously stimulated. The vague hypothesis of a reciprocal interchange of energy between the various brain centers which was there sketched demanded an increase in auditory acuity when auxiliary visual excitation is produced. It was to see if this logical consequent could be experimentally demonstrated that the present investigation was arranged.

Measures of auditory acuity are notoriously subject to grave technical difficulties, but since the most feasible line of attack in this case seemed to require merely the determination of tonal thresholds under two contrasted conditions, the Seashore musical records for pitch and intensity were finally chosen. By simply comparing the percentages correct on both tests under conditions of high general illumination with those secured when the subjects recorded their responses in relative

darkness, some notion of the effect of the brightness stimulation could be obtained. If the hypothesis upon which this work is based be valid, the 'light' averages should be generally higher than the means of the 'dark' responses.

A small laboratory research room with light tan walls was the scene of these activities, all of which took place at night, the necessary 'dark' condition being secured most expeditiously in this manner. The method of applying the visual stimuli varied from time to time (as described in Procedures, *A*, *B*, and *C*, below), but in general, the subject was given two minutes to adapt his eyes to either the light or darkness, depending upon the nature of the session. An ordinary phonograph, manipulated by the experimenter, was placed on a long table with the subject seated in a favorable constant position about a yard away. All tests had to be taken individually; unless otherwise stated, the administration of the records adhered to the standard Seashore routine.

Procedure A.—The high illumination total of 510 watts was secured by means of seven bulbs (two 100-watt, two 75-watt, two 60-watt, and one 40-watt) fairly evenly distributed so that one hung behind the subject and three on each forward side. All were suspended from the ceiling at a distance of about four feet above the subject's head when seated. The uniform brilliancy of the room was enhanced by placing a white oilcloth cover upon the table. 'Dark' conditions were produced by simply extinguishing all lights and eliminating the oilcloth. However, in order to make it possible for the subject to record his responses, a ten-watt flashlight throwing a limited circle of light was fixed in a holder about one inch above the data sheet upon which the judgments were entered. Consequently, the records obtained by this method do not give a true contrast between extremely light and extremely dark surroundings, but only between strong and faint illumination.

The order of test administration was as follows: The pitch and intensity records were first played in the darkened room and then repeated in the lighted one. The pitch record was the starter on the first night followed by the intensity record; on the second night, the order was reversed with the light condition preceding the dark and the intensity score obtained before the pitch data. Twenty undergraduates served as subjects in this series which is the main one of the study.

Procedure B.—The intensity record alone was used on this occasion with ten graduate students acting as observers. Without altering the total wattage of the room, its effective brightness was increased by placing one of the 100-watt bulbs about thirty inches before the subject's eyes. This produced a little initial discomfort but an adjustment was readily made by all subjects involved. For one-half of the group, the order of presentation was 'dark-light-light-dark'; for the other half, 'light-dark-dark-light.' In the final computations, of course, similar conditions were combined.

Procedure C.—The pale light cast by the ten-watt recording bulb seemed to prevent the attainment of the sharpest opposition between light and dark conditions—

hence it was eliminated in this procedure from the subject's field of view. *The experimenter now recorded the judgments as soon as the subject pronounced them orally.* In this case, the subject was in as nearly absolute darkness as could be produced, but a vocal effort was now required. Twenty graduate students participated in this session, ten In the sequence D-L-L-D, and ten in the L-D-D-L. The pitch and intensity records were both played again this time.

III. Tabular Results

For the sake of convenient comparison, the findings obtained by the use of all three procedures are combined in one table below. The scores are all in terms of percent right.

The statistical treatment here followed shows that the mean auditory efficiency as measured by procedures *A* and *B* is definitely better by about three percent in the light than in the dark. In absolute amounts these differences are small but they meet the conventional tests of reliability. Note, however, that in Procedure *C* (which required a vocal effort on the part of the subject) the effect is almost as sharply reversed in the opposite direction in favor of the dark. The influence of the reporting response has apparently been decisive under these conditions.

It is interesting to see how the variabilities check the indications of the means in all cases. Under the first two procedures where the light proved superior to the dark, the light sigmas are also smaller, suggesting greater ease and certainty of discrimination when such conditions obtained. In the last case which favored the dark, the dark deviations are likewise smaller.

The high positive correlations secured between the scores under the two circumstances imply that the improvement under the light condition was fairly uniform for the group studied.

IV. Conclusions

Any interpretation of the findings just presented must be made hesitantly, since they involve rather fine differences and are sorely in need of independent confirmation. Most subjects manifest the effect described, but in a few people the light produced a zero or even inhibiting result upon the auditory discrimination. These exceptional observers were studied

TABLE I

DIFFERENCES IN AUDITORY SENSITIVITY AS MEASURED BY THE SEASHORE PITCH AND INTENSITY RECORDS UNDER LIGHT AND DARK CONDITIONS

Procedure	N	Record	Means		Sigmas		Difference Between Means	Sigma of Average		S.D.diff.	Critical Ratio	Chances in 100 against Reversal	Favor	r
			Light	Dark	Light	Dark		Light	Dark					
A (510 watt illumination vs. 10 watt guide light)	20	Pitch	78.600	75.675	8.62	9.79	2.925	1.215	1.380	0.572	5.114	100	Light	0.91
	20	Intensity	87.900	84.975	6.03	8.36	2.925	1.061	1.471	0.778	3.760	100	Light	0.86
B (same as *A* but with fixation upon 100 watt light)	10	Intensity	89.150	86.050	3.57	5.97	3.100	1.153	1.928	1.438	2.156	98	Light	0.67
C (subject utters responses in both complete darkness and full light)	10	Pitch	73.850	75.400	12.09	10.80	−1.550	3.530	3.154	2.481	0.625	73	Dark	0.73
	10	Intensity	87.850	88.500	6.12	4.01	−0.650	1.083	0.710	0.541	1.201	88	Dark	0.90

further in the hope that some constitutional factor might be found to explain their behavior, but none was detected. It may be of some significance to note that the earlier work of Urbantschitsch also makes mention of these idiosyncratic observers.

The best that one can say is that most adult subjects are capable of somewhat finer tonal discrimination under bright illumination than under dim or dark conditions. There is also some indication that persons whose normal performance is relatively poor are the ones in whom the visual facilitation is most marked. The observers claimed that they felt more alive and awake under the light, and it is possible that this energizing influence of the strong illumination upon the total system is primarily responsible for the result. By toning up the conscious organism and making it more alert to all environmental stimuli, the light apparently succeeds in enhancing the sensory efficiency of the hearing tracts.[11] Some may prefer to term this a consequence of heightened attention, but that is a matter of indifference to the present writer.

There is the faint possibility that for some subjects a native or acquired fear of the dark may lead to a mild emotional excitement which interferes with the best functioning of the discriminating mechanism. However, it is improbable that this could have been the decisive influence, since in procedure *C* (when the subject talked) the dark condition was actually superior.

At first blush, these results appear to clash with the well-established psychophysical generalization known as Heymans' law of inhibition, which states that the inhibitory power of a stimulus, measured by the intensity of a second stimulus which it can just completely inhibit, is directly proportional to its intensity. This means that a simple threshold is raised by the presence of a second stimulus in the environment. An

[11] In his Influence of color upon mental and motor efficiency, Pressey had reported a dynamogenic effect of bright light upon several performances. Most of his subjects showed an increase in tapping rate, rate of multiplying, and rate of free association under bright white light and a decrease in dim light. His observers claimed that the bright light invigorated them but that the dim light made them feel sleepy. *Cf. Amer. J. Psychol.*, 1921, **32**, 326–356.

everyday illustration is the fact that the sensory experience produced by a few hundred milligrams of pressure vanishes if a kilogram weight is applied to the skin nearby. There seems to be no reason for questioning the validity of this law since it has been repeatedly verified for both absolute and differential thresholds within the same sensory field.[12] On the other hand, we know little of its applicability to the sphere of intersensory phenomena with which this paper is concerned. To be sure, Jacobson's [13] interesting 'Experiments on the Inhibition of Sensations' had demonstrated the inhibition of pressure sensations by simultaneously aroused sound sensations, and *vice versa*—a fact which evidently stands in flat contradiction to the recent discoveries of intermodal facilitation.

Since Jacobson offers an interpretation of his work in terms of attention, a hint of the possible reconciliation may be developed here. Tentatively, it may be said that a simultaneous auxiliary heteromodal stimulus *augments* the main stimulus when the former is in the *background*, but that it acts in an *inhibiting* way when it is *focal*. As *figure*, the auxiliary stimulus would raise thresholds in accordance with conventional expectations of the results of distraction, but as *ground* (with the main stimulus in the rôle of 'figure') it may well produce facilitation. Whether this broad guess be found to fit the situation or not, the most conservative hypothesis for the present issue seems to be that the neural activity originating in the occipital lobe spreads to other receptor areas of the cortex (particularly the temporal section) and by subexciting those regions before or during specific stimulation increases the phenomenal intensity of the auditory experiences.

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[12] See L. T. Spencer, The validity of Heymans' law, *Amer. J. Psychol.*, 1925, **36**, 427–433.

[13] Cf. *Psychol. Rev.*, 1911, **18**, 24–53.

ON 'LEARNING WITHOUT AWARENESS OF WHAT IS BEING LEARNED'

BY F. W. IRWIN, K. KAUFFMAN, G. PRIOR, AND H. B. WEAVER

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Thorndike and Rock[1] have offered evidence that "a satisfying after-effect could strengthen the connection which it followed and to which it belonged in cases where the learner did not know what the connection was." The value of this evidence, however, rests upon an assumption which seems open to question, namely, that lack of awareness of what was being learned was indicated by continued slow 'learning' without a sudden increase in the percentage of successes. This point can best be made clear by a description of the procedure of the experiment in question.

The instructions to the subjects were as follows:[2]

"'I shall say 'Ready' and then say a word. You will say the first word or words that you think of. I will record the time in fifths of a second. Then I will say another word. You will say the first word or words that you think of. I will record the time as before. We will continue this. At the end of the experiment, you will receive a money bonus depending upon how quickly you respond on the average. Also I shall say 'Right' if the word that you say is one of ten or more that we have arbitrarily decided to call right as responses to the word in question. I shall say 'Wrong' if the word that you say is one of ten or more that we have arbitrarily decided to call wrong as responses to the word in question. Sometimes I shall say nothing because the word that you say is in neither our list of Rights nor our list of Wrongs for the word in question. At the end of the experiment, you will receive a money bonus depending on a system of credits for Rights and penalties for Wrongs. Speed is the main thing. The number of Rights versus Wrongs is a minor thing.'"

A number of words were then read to him, one at a time. "In each case, if his response is clearly due to the sequential connections used in speaking and writing · · · he is rewarded by the announcement 'Right.' If his response is

[1] E. L. Thorndike and R. T. Rock, Jr., Learning without awareness of what is being learned or intent to learn it, J. EXPER. PSYCHOL., 1934, 17, 1–19. Quotation from p. 1. It will be noted that of the two experiments reported in the article cited the present paper concerns only the first.

[2] *Ibid.*, pp. 1–2. The last two sentences are from the note on p. 9. Professor Rock informed us that these sentences were a part of the original instructions as given.

clearly due to connections used in getting the word's meaning . . . he is punished by the announcement of 'Wrong.'" [3] 'Learning' was indicated by an increase in the number of correct responses given by the subject as the experiment progressed. "If the subject became aware of the principle according to which the rewards and punishments were allotted or of the two classes of connections, he would, after a little experimentation, change from a moderate percentage of successes to nearly 100 percent. If the subject increases his percentage of responses that manifest the one tendency gradually, it is evidence that that tendency is strengthened without his being aware that there is any such tendency." [4] It was to these latter statements that we took objection, since they are presented as being obvious, and are not supported in any way either by logic or by experimental results. Since Thorndike and Rock believe (and we agree with them here) that the problem attacked by their experiment is an important one, it seemed worth while to repeat it with control over this fundamental assumption. Our problem may be stated as follows: Is it not possible that a gradual increase in the percentage of correct responses may take place even though the subject is perfectly aware of the distinction involved? If so, then such a gradual increase cannot be used as a criterion of lack of awareness of this distinction.

Every effort was made to keep the procedure of our experiment exactly comparable to that of the original [5] except in the case of deviations necessary for the carrying out of the controls. All such deviations are made plain in our statement of procedure. Twelve subjects, 7 men and 5 women, were employed, all of whom were between 20 and 30 years of age. Each subject at some time during the experiment was *taught* the distinction involved in the announcements of 'Right' and 'Wrong.' In 4 cases (subjects A, D, E, and G) this teaching occurred early in the experimenting

[3] *Ibid.*, p. 2.

[4] *Ibid.*, p. 3.

[5] The authors wish to acknowledge the gracious assistance of Professor Rock, who kindly supplied a list of the words used in the original experiment and gave other information about certain details of the procedure.

(after 10–20 responses); in the other 8 cases (B, C, F, H, I, J, K, and L) it occurred after 100–120 responses. After the two associative tendencies had been explained carefully to the subject he was asked to tell which tendency was operative in several sample words and responses that were shown to him; this was done in order to be perfectly sure that the subject fully understood the distinction being made. The experiment was then continued in the usual manner. At the end of each subject's performance (except in the cases of A and B, the first two subjects, and H, J, and K, who ended their regular series with almost perfect scores) he was given 20 words to which he was instructed to respond as carefully as he could, taking his time and not necessarily giving the first word that came into his mind. This served as a check upon the distinction's still being remembered at the end of the experiment, and reference to these scores in the results (indicated in Table I by numbers in bold-face type) will show that in every case the subjects at the end of their experimental periods were well aware of the distinction. Reaction times were taken with a stop-watch; they were used to prevent the subjects' lengthening their time materially, particularly after having been taught the distinction. In several cases subjects were reminded that "speed is the main thing."

The results presented in Table I demonstrate that in the case of those subjects whose learning of the distinction *by being taught it* was deferred until the middle of the experiment a gradual increase in the number of correct responses in successive tens usually occurred; this fully agrees with the results of Thorndike and Rock. On the other hand, and most important for the point of the present paper, knowledge of the distinction *does not* in general lead to a change "from a moderate percentage of successes to nearly 100 percent," even with a liberal interpretation of "nearly." This is shown by the gradual learning of the subjects *after teaching*, whether the teaching took place early or late in the experiment. The most striking changes which could be interpreted as due to knowledge of the distinction were found in the cases of H, J, and K. H announced at the end of 80 words that she

TABLE I

NUMBER OF RESPONSES ANNOUNCED AS 'RIGHT' IN SUCCESSIVE TENS FOR INDIVIDUALS A, B, C, ETC.

Each individual's series is divided into two parts by a horizontal line indicating the point at which he was taught the distinction. Bold-face numbers indicate test trials at the end of an individual's series.

	A	B	C	D	E	F	G	H	I	J	K	L
I	6	5	2	3	4	8	2	3	1	1	3	2
II	8	3	6	7	4	10	3	1	3	3	8	7
III	8	4	3	4	3	8	3	3	6	2	9	2
IV	5	5	1	5	3	7	3	6	4	2	9	3
V	8	4	4	6	5	6	1	7	8	4	9	2
VI	9	6	3	5	5	9	3	5	5	2	8	3
VII	9	5	5	9	6	9	4	5	5	4	7	1
VIII	10	3	9	5	5	9	3	9	7	3	9	2
IX	9	6	8	6	10	8	2	9	7	0	5	1
X	8	6	7	4	3	7	4	7	7	5	6	3
XI	9	6	6	8	4	8	4	7	7	6	10	7
XII	9	4	5	6	3	8	2	3	7	3	10	2
XIII	10	4	9	5	8	9	4	9	8	8		5
XIV	10	8	7	5	4	10	4	9	9	10		6
XV	10	8	8	7	3	9	6	8	9	10		5
XVI	10	6	10	7	6	8	4	10	10	10		1
XVII	10	6	9	6	4	9	0	9	9	8		5
XVIII	10	7	6	5	5	10	5	8	7	6		6
XIX	9	6	7	7	6	9	2	8	7	9		7
XX	10	6	8	6	5	7	4	7	7	7		3
XXI		4	**9**	**10**	**10**	**9**	**10**	8	10	9		**9**
XXII		5	**10**	**10**	**10**	**10**	**10**	9	9	10		**10**
XXIII		5						8	9			
XXIV		4							7			
XXV		4							**10**			
XXVI		5							**9**			
XXVII		5										
XXVIII		7										
XXIX		6										
XXX		7										
XXXI		8										

believed she knew the criterion of correctness of response, which, upon her being asked to state it, was found to be true. She then went on, and during the next 40 words rapidly *lost* the efficiency at which she had previously arrived. Then upon being taught the distinction anew, she immediately regained a relatively high level of correctness which was maintained to the end of her experimental period. K announced her knowledge of the distinction at the end of the first 10 words, after which several series of 10 words showed a high degree of correctness followed by a general decrease.

Upon being taught the distinction this subject immediately gave 20 correct responses in succession. Against this possible evidence of the effect of such knowledge are a number of subjects who seem to show no traces of such an effect (B, C, D, E, F, G, and L). These subjects seem to learn, if they learn at all, no more rapidly than did the subjects of Thorndike and Rock who had never been taught the distinction. The results as a whole, then, are definitely contrary to the expectation that knowledge of the distinction will soon lead to near perfection in response. When it is considered that a single unfavorable case is enough to throw upon Thorndike and Rock the entire burden of proof that their subjects were learning without knowledge of what was being learned, we believe it necessary to conclude, in the absence of such proof, that their 'Association Experiment' affords no evidence of such learning without knowledge.

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THE EFFECT OF RECALL ON FORGETTING[1]

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In experiments relating to the effect of recall on learning the procedure is essentially to vary the presentation of the learning material so that the amount of recitation is controlled. Under these conditions most investigators, *e.g.* Abbott (1) or Trow (7), have found that learning is more efficient with recitation than without it, and have offered two general types of explanation for this finding. According to the first, represented by Skaggs (5), recitation shows a subject where his learning is incomplete, and thus gives him greater opportunity than reading to check on his errors. It also offers him more incentive to perfect his learning, and may afford less retroactive inhibition. The second explanation, suggested by Gates (3), among others, depends on the fact that learning is tested by reproduction. Since recitation involves reproduction, the significant responses are practised in recitation more than in reading, and this exercise is considered to be the most important factor concerned in the effect of recall.

It would be very difficult to find a crucial test of these theories when recitation is introduced into the learning period. In the present experiment the learning period was the same for all subjects; recalls were introduced during the seven days following it. Evidence will be presented which indicates that neither of the explanations suggested is adequate to account for all the effects of recall.

Procedure

The learning material was a list of 100 familiar words, from one to four syllables, and three to ten letters in length, including nouns, verbs, adjectives and adverbs. In

[1] This experiment has benefited greatly from the advice of Dr. Raymond Royce Willoughby, whose criticisms helped to clarify the problem and to define the procedure.

[2] From the Psychological Laboratories of Clark University.

preliminary tests it was found that, although the words were relatively unselected, certain groupings could readily be formed, such as *farmer-cabin-calico*, or *philosophy-history*. The words were written on separate slips of paper, mixed, drawn out singly, and arranged in the order in which they had been chosen. In a few cases in which very similar words occurred together, changes in order were introduced.

The exposure device was a simple tachistoscope, consisting essentially of a white enamel frame on a wooden base. The angle between the base and the frame was adjustable. A slip of paper inserted in the frame could be drawn through it so as to be invisible to the subject except for a small rectangular opening, in which the words were exposed. The experimenter determined the length of the exposures according to the ticking of a metronome: each word was shown 1½ seconds, and the time for the entire list was 2½ minutes. The directions to the subject were, "Now I am going to show you a list of 100 words. I'll show it five times, and of course you won't be able to remember them all; but try to remember as many as you can. The order they come in is of no importance." The short interval between exposures was filled with conversation. This unit learning period was the same for all subjects.

In preliminary tests, the list was shown to the subject, who was then told to write as many words from it as possible. A free association was given, and it was found that very few of the words from the list were included. Since the free association method offered advantages not found in the more customary tests of recall, it was combined with reproduction in this experiment, and the following directions given the subjects, immediately after exposure of the words: "Now I want you to say 150 words, more words than there were on the list. Try to say words from the list, but if you think of a word say it, even if you know it was not on the list. For instance, if you feel yourself on the track of a word but think of others in the meantime, say the others too. Let me know when you've said a word from the list, or if you're doubtful about a word." A record was kept of all the words spoken, and of the degree of recognition. If the subjects believed they could remember more words after they had said 150, they continued until they considered their reproduction as complete as possible.

The only disadvantage in this method was found in the case of a few experienced subjects, for whom it was difficult at first to combine reproduction and free association, because of their familiarity with one or both. This difficulty usually vanished quickly; and it did not appear in the majority of cases. The advantages of the method are:

1. It offers some control of the minimum time spent in reproduction, but none of the maximum.
2. It indicates what associations have been formed among words on the list, or between those words and others.
3. It shows to some extent the ease with which words were remembered.
4. Perseveration during the period of reproduction is shown by repetitions.
5. The subject is apt to report words of which he is doubtful.
6. An opportunity is offered for the reproduction of words that cannot be recognized.

This method of recall was used throughout the experiment; and one such test was always given immediately after learning. For subsequent recalls the instructions were: "This is just like the last time. Say 150 words; try to say words from the list; but actually say any word you think of. Let me know when you've said a word from the list, or when you're doubtful about a word."

At the end of the first experimental period, instructions were given not to try to recall words on the list except during the experiment. All of the subjects reported

later that there had been no voluntary recall; a few said that words had perseverated. This was never reported for periods later than the first day.

Forty-eight subjects were used, all adult, coöperative, and with more than high school education. In an experiment of this type, when the periods tested are relatively long, adequate control of retroactive inhibition is impossible. A rough control was offered by the fact that all the subjects were either employees or students. The subjects were divided into four groups of twelve, and given retention tests as described in Table I.

TABLE I

SCHEDULE OF RECALL

	1st Day, Immediately after Learning	2d Day	3d Day	4th Day	5th Day	6th Day	7th Day
Group	A	—	—	A			
Group	B	—	—	—	—	—	B
Group	C	C	C	C	C	C	C
Group	D	D	—	—	D	D	D

Every word on the list that was reproduced correctly was given a score of one. If a subject reported a word similar to one on the list and designated it as being on the list, it was assigned a score of one-half; if he did not report it as being on the list it was given no credit, whatever the degree of similarity. Examples of words given half credit are *philosopher* for *philosophy*, or *imply* for *employ*. Synonyms were given no credit, since the problem was one of recall of words, not of meanings. Because of the large number of data and the lack of homogeneity in the original material, it was considered preferable to have such an arbitrary but consistent and simple method of scoring as this, rather than one that was more complex and therefore more subject to the interpretations, and possible bias, of the experimenter.

RESULTS

Because no recalls were introduced until the learning period was over, the learning of the different subjects could not be equated. For group comparisons, therefore, the score

made on the first retention test was taken as total learning for each subject, and all his later scores expressed as percentages of it. When a curve of forgetting is constructed on this basis, as in Graph I, using the mean scores of the group, the typical

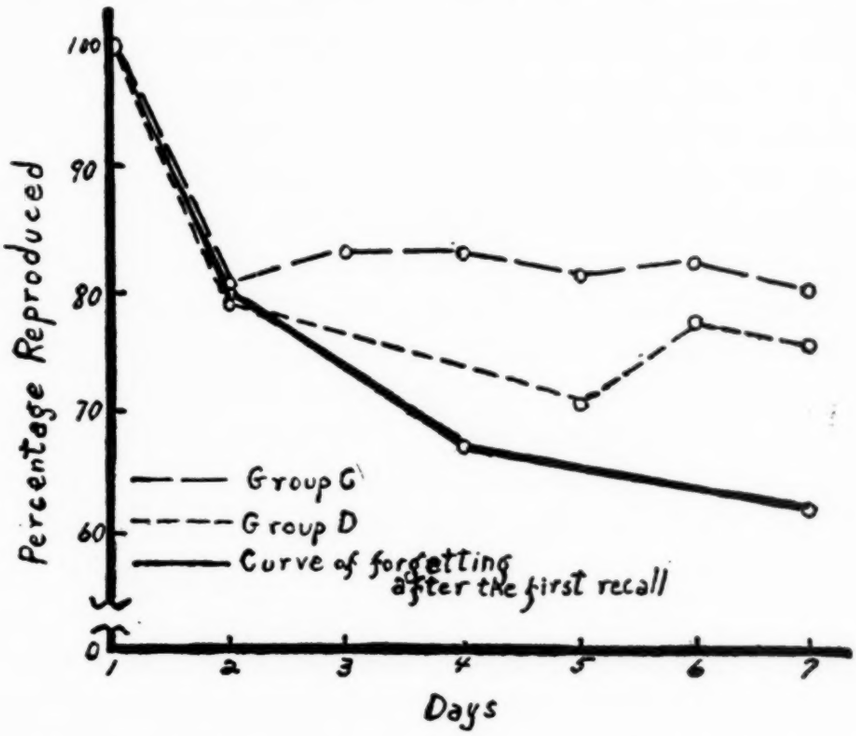

FIG. 1. Mean scores of each group, expressed as percentages of the number of words reported the first day.

negatively accelerated curve of forgetting is found. Comparison of Groups C and D shows that on the second day, when experimental conditions were the same for both, the mean score of one was 79 percent and of the other 80 percent. This indicates that the material is highly reliable, since the data of one group are so nearly duplicated by the other. If median scores are found instead of mean scores, the points of Graph I are almost exactly duplicated. The distributions, therefore, were not badly skewed, although the number of subjects was relatively small.

The validity of the material is shown by the similarity between this and the customary curve of forgetting, found with different learning material, method and degree of learning, or method of recall and scoring. Since none of these factors, or

other constant experimental conditions, gave rise to unusual results in this part of the experiment, it is improbable that any unusual features in the retention scores for groups with repeated recall could have been due to them alone. There is strong evidence, therefore, that the only relevant factors in interpreting the results, are the number and distribution of previous recalls, in relation to the time elapsed since learning.

For Group C, with daily recalls, the mean scores from the third to the seventh day were never quite so low as on the second day, but always at approximately the same level; and differences among them are well within the range of experimental error. The differences between the score on the first day and the score on each of the following days are statistically significant; all of the differences after the first day are insignificant. The effect of daily recall, under these conditions, is therefore clear; it retards forgetting beyond the second day, within the limits of the experiment, and probably has a tendency to raise scores slightly above the second day level.

The data of Group D are more complex. Recall on the fifth day showed a score significantly lower than on the second; on the sixth day there was a group rise in score, significantly higher than on the fifth, and practically at the level of the second. The score the following day was somewhat lower, but the meaning of the drop is ambiguous; since the experiment was not continued further, it may be interpreted either as the beginning of a new trend or as a chance irregularity in the data. The rise on the sixth day shows that the effect of recall is not simply to retard forgetting at the level at which it is introduced, but that under certain conditions it may act to produce reminiscence; such an effect, if recall was introduced into the learning period, would be interpreted as accelerated learning due to recitation. The slight drop in score on the seventh day, compared to the more uniformly high scores of Group C, indicates the instability of the material.

Probably the most interesting trend found on analysis of individual scores is related to reminiscence. No subject reported his maximum number of words after the second day in Group A, Group B or Group D; that is, there was no rem-

TABLE II

SIGNIFICANCE * OF THE DIFFERENCES BETWEEN RECALLS
Italic = significant

	2d Day	3d Day	4th Day	5th Day	6th Day	7th Day
Group A						
1st day.....			*.999*			
Group B						
1st day.....						*.999*
Group C						
1st day.....	*.999*	*.999*	*.998*	*.999*	*.999*	*.999*
2d day.....		.858	.853	.688	.802	.667
3d day.....			.500	.707	.588	.854
4th day.....				.809	.583	.805
5th day.....					.603	.594
6th day.....						.904
Group D						
1st day.....	*.999*			*.999*	*.999*	*.999*
2d day.....				*.999*	.719	.920
5th day.....					*.986*	.938
6th day.....						.807

* *P* found from 'Student's' formula (**6**).

iniscence when more than one day elapsed between successive recalls. There was one reminiscent in Group D on the second day; in Group C there was one on the second day, another on the third and still another on the fourth. This corroborates the group data in suggesting that repeated recall has a reinforcing effect on memory, and may under certain conditions facilitate reminiscence.

Analysis of the words reported correctly by each subject shows that reminiscence, not in total score but in the items that compose it, occurred frequently. The top horizontal line of Table III gives the mean number of words reported for the first time in each recall; there is a steady decrease except for Group D on the sixth day, the day that there was a rise in the total score of the group. This indicates, though the evidence is tenuous, that no clear distinction need be made between processes at work in words that had already been recalled, and those that had previously been below the threshold; for the only day on which there was no decrease in words reported from the second of these groups was also the only day on which

there was a statistically significant increase in the number of words reported from the first.

The lowest diagonal line of each group shows the number of words that were not forgotten in any recall; this number, also, decreases consistently. Since the diagonal next to the lowest shows an increase, in Groups C and D, it is apparent that the words which were repeated most often, though they might be forgotten one day, would probably be remembered on the next. Such irregularity in reproduction was not unusual; occasionally a word seemed on the threshold as the term is used psychophysically, for it might be remembered, for example, on the first, fifth and seventh recitations, and on none of the others.

TABLE III

MEAN NUMBER OF REPETITIONS OF EACH WORD

	Number of Times Reported	1st Day	2d Day	3d Day	4th Day	5th Day	6th Day	7th Day
Group A								
	1	36.5			2.6			
	2				22.1			
Group B								
	1	41.7						1.9
	2							24.6
Group C								
	1	43.2	3.8	2.8	1.8	1.1	.5	.4
	2		30.9	4.5	2.8	1.4	1.2	.6
	3			28.8	4.3	2.8	1.5	1.2
	4				27.2	4.7	2.7	1.2
	5					25.3	5.1	2.2
	6						24.8	5.3
	7							24.2
Group D								
	1	35.0	3.5			1.4	1.5	.9
	2		24.2			2.8	1.8	1.0
	3					20.7	3.5	1.5
	4						20.4	3.7
	5							19.4

One of the most interesting results of the experiment, and certainly the most unexpected one, was the reproduction of many words that were not recognised. I have found no previous experimental reports of this, although Hollingworth

(4) has suggested that it might occur, on the evidence offered by dreams and involuntary plagiarism. The more customary opinion, expressed, for example, by Achilles (2), is that the difference between recall and recognition is a matter of degree, so that material which was relatively poorly learned may be recognized; if it was learned somewhat better it may be both recognized and recalled; but recall without recognition is impossible.

When, as in this experiment, the learning material is composed of elements that are already familiar to the subject, it is difficult to determine whether a word reported in the association test and not recognized was spoken by chance, or was an example of actual recall without recognition. There are several indications that chance was not the only factor involved, and although any one of them may be inconclusive, taken together I believe that they constitute a strong body of evidence for the fact.

1. Sometimes a subject hesitated about a word he had said, and then decided he did not recognize it; in this case it could hardly have been recalled by chance.

2. A word reported and not recognized might be repeated and recognized in the same association.

3. Recognition of a word might change from one report to the next. To give two examples of many; one subject reported but did not recognize a word in her first recall. She repeated it on the fourth, and thought it might be from the list; she repeated it on the sixth, without any recognition. Another subject recalled and recognized a word on her first test, reported it for the second time, without recognition, on the fifth, reported it on the seventh test and was doubtful about it. Similar transitions were frequent.

4. Pronounced individual differences were present, which should not have occurred if words from the list were said only by chance. To consider the subjects of Group C, on whom there are most data, one reported but did not recognize only one word in her seven recalls, another only three; while two other subjects did not recognize twenty-eight and thirty-one words that they reported correctly.

5. The last fact to be cited is of a different sort, and either irrelevant, or by far the most important type of evidence. In a large number of cases, these words were given, not intermittently during the course of the association, but at what seemed a crucial point. Often, for example, a subject would report that he could not remember any more, then almost immediately recall a group of words. Occasionally, after he said that his recall was complete, he would report one or two words that he did not recognize. Again, in the most typical cases, a subject recalled immediately about three-quarters of the words he knew, and the remaining words would be given in small groups as they were suggested by associations, or recalled without obvious cues. Often words that were not recognized would be reported at the beginning or end of these groups, although no association between them was apparent to the experimenter. Such evidence is, of course, too slight for theorizing; it is suggestive, however; and further experiments may show that it is significant.

Discussion

When recall is substituted for reading during learning, the learning is usually facilitated; when similarly, there is repeated recall after learning is completed, more is remembered than when there has been no recall. The only possible explanation of these facts, in the light of the more rigid stimulus-response theories of behavior, is that the effect of recall is due to practice; responses which have already been learned become stronger through repetition. This cannot account, however, for the group rise in score due to recall, nor for the fact that only subjects with daily recalls showed reminiscence; such findings indicate a facilitatory effect of recall which acts on material that was not reported as well as on material reproduced.

Certain generalizations seem justified by this experiment, expecially as it is corroborated by much other work: to account for reminiscence some trend must be assumed which is positive as forgetting is negative. Both of these trends are probably present in partially learned material; recall aids the

positive one. If it be assumed that the equilibrium between the two processes makes the material more stable at certain times than at others, the different effects of recall introduced at different intervals become explicable; such hypotheses of systems of material at different stages of equilibrium can be applied readily to the facts of transfer and interference, as well as to the data of reminiscence. If, then, the facilitating effect of recall is stated in terms of a process at work on all the learning material, the facts are subject to a ready explanation. Such an explanation can not be supplied if the material is analyzed into the simpler units of individual words, which correspond to the divisions of stimulus and response, and their connecting bonds.

Summary

A list of familiar words was partially learned by four groups of twelve subjects. Under these conditions:

1. The typical curve of forgetting was found.
2. The effect of daily recall was to retard forgetting after the second day.
3. Under certain conditions, with recall on successive days, there was a statistically significant group increase in number of words remembered.
4. Reminiscence was found only in subjects who recalled the material on successive days.
5. Even where the total score did not show reminiscence, words were often reported for the first time after the first day.
6. The number of words reported for the first time decreased daily, except when there was a group rise in score.
7. Words were recalled without recognition.
8. Recall probably acts upon the learning material as a whole, and not only upon the units that are reproduced.

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MEANING VERSUS REPETITION, RECENCY, AND EFFECT IN THE DETERMINATION OF CHOICES OF ALTERNATIVE LETTERS OF A MENTAL MAZE

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Introduction.—The present study was performed with the primary intention of attempting to discover further evidence upon three vital issues involved in the psychology of learning. First, it was desired to know whether meaning or the combined effects of repetition, recency, and satisfaction or annoyance were the more important in determining the directional aspect of learning behavior. Exactly what meaning is, probably no one at present can tell us. It appears, however, to be closely allied with use, and to result from an analysis or perception of relations within a problem, relative to the end toward which the individual is striving. It may also be identified with insight, in that it represents an organized response directed toward a goal. It is assumed, with respect to the Law of Effect as stated by Thorndike, that the choice or avoidance of an alternative response would be in agreement with the corresponding effects of satisfaction and annoyance. It is further assumed that any repetition of a choice of an alternative response would be in harmony with principles of frequency, recency, and satisfaction. Secondly, it was desired to know whether individuals would respond similarly to identical elements in varied relations. An identical element is here conceived as any part or aspect of a situation which can be perceived as a whole, and which may appear in unchanged form upon a varied background. Thirdly, it was desired to know whether the combined effects of repetition, recency, and satisfaction or annoyance, in a problem where these laws would presumably be opposed to learning, would tend to prevent learning.

The Problem.—The problem was to study the possible combined importance of repetition, recency, and effect in the determination of choices of letters of a mental maze when such letters were arbitrarily designated as incorrect at each fourth alternative. The second objective was the comparison of the relative importance of perception of relationships as a factor in the determination of choices with the above principles of learning. The third aspect of this problem was the observance of characteristic behavior patterns relative to the emergence of perceptions of relationships.

The Maze.—The maze, constructed for this study, was similar to that devised by Peterson.[1] The present maze contained twenty-five alternative choices of letters. Eight of these letters were termed significant, because each appeared four times in succession at successive stages with one other alternative letter which never appeared but once, and because the significant letters were always designated as correct for the first three alternative steps at which any one of these letters occurred, and incorrect upon the fourth step. The scheme to be grasped by the learner consisted in the choice of the significant letters three times in succession, and in the choice of the other alternative at the fourth respective step. The clue to the perception of this general relationship lay in the fact that any incorrect choice threw the learner back to the starting point to begin over. It was necessary for the learner to conclude that incorrect choices led back to the starting point, since no obvious indicator of this fact was present.

The letters of the maze, indicating alternative pathways, were called out to the observer in pairs. Whether the correct or incorrect letter was called out first or second in sequence was a matter of chance. The chance order was obtained by flipping a coin and inserting an artificial stencil containing the order of heads and tails into a Pressey Calculating Machine. Errors made on different steps effected a constant and additional shifting in the order in which any letter of a pair was called out to the observer. A screen separated the observer from the experimenter.

The Procedure.—The observers for this experiment were selected at random from an undergraduate class in Educational Psychology.[2] The directions to observers were typewritten. The general nature of a maze was explained. It was stated that the experimenter would call out two letters at a time, and that after each stimulus the observer was to choose one letter and call it back to the experimenter. It was made clear that variations in emphasis, and the order of sequence in calling out the letters of any pair were not to be regarded as clues to the solution. It was further pointed out that choices made with respect to preferences, order of appearance of letters in the alphabet, mathematical orders of sequence, or the construction of words would prevent any arrival at the solution to the maze. The experimenter called out the alternatives as rapidly as the observer responded without any obvious indication as to progress or failure, and continued thus for one hour, unless the observer arrived at the solution.

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[1] Peterson, J. Learning when frequency and recency factors are negative, *J. Exp. Psychol.*, 1922, **5**, 270–300.

[2] At the State University of Iowa.

The Pressey Calculating Machine was operated with the left hand following each stimulus, and the order in which the letters of the next pair were to be called out could be read from the dial. Each stimulus was recorded by the experimenter just as it was given to the observer. The choice of any particular letter made by the observer could be determined by the next subsequent stimulus as recorded, since where the observer proceeded in the maze depended upon the correctness or incorrectness of the immediately preceding choice. This procedure simplified the method of recording the data, and at the same time permitted a relatively easy analysis of each response.

The Data.—Twenty-eight observers were employed in this experiment. The data consisted of an analysis of choices of significant letters of the maze when such letters always constituted the correct alternative three times in succession, but were incorrect at the fourth respective alternative steps. The results for observers solving the maze will be found in Table 1, Part A. The corresponding results for observers not solving the maze will be found in Table 1, Part B. It was considered important to analyze the data for these latter observers in order to attempt to discover differences in be-

TABLE 1

AN ANALYSIS OF CHOICES OF SIGNIFICANT LETTERS OF A MENTAL MAZE

Part A. Observers Solving Maze

When the Significant Letters Were:

	Correct		Correct		Correct		Incorrect	
	First Alternative		Second Alternative		Third Alternative		Fourth Alternative	
	Total Number of Choices	Percent Choices of Significant Letters	Total Number of Choices	Percent Choices of Significant Letters	Total Number of Choices	Percent Choices of Significant Letters	Total Number of Choices	Percent Choices of Significant Letters
I*	1880	51± 1.1†	966	50± 4.6	479	59± 2.2	282	27± 2.6
K	282	73± 2.6	206	81± 2.7	167	79± 3.1	132	15± 3.0
E	132	85± 3.0	112	92± 2.5	103	82± 3.7	84	9± 3.1
B	84	91± 3.1	76	93± 2.9	71	86± 4.1	61	10± 3.8
T	61	90± 3.8	55	96± 2.6	53	87± 4.6	46	11± 4.6
C	46	89± 4.6	41	98± 2.2	40	90± 4.7	36	11± 5.2
Y	36	89± 5.2	32	91± 5.1	29	90± 5.6	26	4± 3.7
S	26	96± 3.7	25	96± 3.8	24	92± 5.5	22	18± 8.1

Part B. Observers Not Solving Maze

I	4828	42± .7	2023	33± 1.0	684	54± 1.9	367	71± 2.4
K	367	29± 2.4	105	60± 4.8	64	67± 5.8	43	19± 7.5
E	43	81± 7.5	35	77± 7.1	27	81± 7.5	20	18± 8.6
B	22	82± 8.6	18	89±10.1	16	81± 9.8	13	18±10.6
T	13	82±10.6	11	91± 8.6	10	100 —	10	20±12.6
C	10	80±12.6	8	100 —	8	100 —	8	25±15.2
Y	8	75±15.2	6	100 —	6	100 —	6	0 —
S	6	100 —	6	100 —	6	100 —	6	100 —

* Significant letters.

† Standard errors of percentages.

havior, as compared with individuals who were successful, which might indicate the fundamental factors causing success or failure. The critical ratios, *i.e.* the differences in percentages divided by the standard errors of the differences, showing the degrees to which the differences in the percentages were statistically significant for the various successive choices of significant letters for observers solving and those not solving will be found in Table 2. The results were further composed of certain general observations made by the experimenter, and of the reports of observers concerning their experience while solving the problem.

The Results.—Eighteen individuals solved the problem, while ten did not reach the goal within the time limit of one hour. It will be seen from Table 1, Part A, showing the analysis of choices for observers solving the maze, that the percentages of choices for each of the significant letters for each of the first three alternatives, when these letters were designated correct, were consistently and approximately the same. With the exception of the percentage of choices of the letter 'I' at the third alternative, as compared with the respective percentages for the first two alternatives, there was no difference which was statistically significant, as can be observed from Table 2. The percentages of choices for each of the significant letters at each respective fourth alternative, when these letters were designated as incorrect, were consistently and significantly less than the corresponding percentages for each of the first three alternatives.

The increase in the percentages of choices of the second significant letter over that of the first for each of the first three alternatives, and the respective decrease at the fourth alternative, was significant. The increase in the percentages of choices of the third significant letter over that of the second for each of the first three alternatives, and the still further decrease at the fourth alternative, was not consistently significant. It may be found that further corresponding increases or decreases for the remaining significant letters of the maze were not statistically significant. It may be observed from Table 1 that the standard errors grow consistently larger from one letter to the next as a result of the decrease in the numbers of choices in the latter steps of the maze, and this may partly account for the disappearance of significant differences. However, it should be pointed out that it is probable

TABLE 2

CRITICAL RATIOS SHOWING SIGNIFICANCE OF DIFFERENCES IN PERCENTAGES OF CHOICES OF SIGNIFICANT LETTERS OF THE MENTAL MAZE

	When the Significant Letters Were:			
	Correct	Correct	Correct	Incorrect
	D/S.E. diff. between the Various Alternatives for Observers Solving Maze			
	First–Second	First–Third	Second–Third	Third–Fourth
I*	−†.51	+3.36	+3.30	− 9.41
K	+2.14	+1.48	− .48	−14.88
E	+1.79	− .63	−2.24	−15.14
B	+ .47	− .97	−1.39	−13.59
T	+1.30	− .50	−1.70	−11.69
C	+1.76	+ .19	−1.54	−12.71
Y	+ .27	+ .13	− .13	−12.81
S	0	− .59	− .59	− 7.53
	D/S.E. diff. between the Various Alternatives for Observers Not Solving Maze			
I	−7.37	+5.94	+9.81	+ 5.55
K	+5.77	+6.06	+ .93	− 5.06
E	− .38	0	+ .39	− 5.53
B	+ .53	− .08	− .56	− 4.36
T	+ .66	+1.69	+1.05	− 6.34
C	+1.59	+1.59	0	− 4.93
Y	+1.64	+1.64	0	− ∞
S	0	0	0	0

* Significant letters.

† Plus or minus indicates an increase or decrease in the percentages from one alternative to the next following as listed.

that most observers perceived the scheme of the maze within the range of the first two significant letters which would prove to be an important factor in the disappearance of differences.

It will be observed from Table 1, Part B, and Table 2 that in general the results for observers not solving the maze were similar to the results for observers solving the maze. Certain differences in these results are important. In the first place, it will be found that 71 percent of all choices made on the first two alternative steps of the maze were made by individuals who were unsuccessful. The percentages of correct choices for these two steps, for observers not solving, were significantly less than the corresponding percentages for observers solving

the maze. This would indicate an unfortunate beginning for the unsuccessful observers. It will be found upon further analysis that 90 percent of the choices made by individuals not sloving the maze were made upon the first four alternative steps, *i.e.* the range covered by the first significant letter, as compared with 64 percent for observers solving the maze. 97 percent of the choices made by observers not solving the maze were made upon the first eight alternative steps, *i.e.* the range covered by the first two significant letters, as compared with 78 percent for individuals solving the maze. It is clear that this condition was the result of persistence in the choices of incorrect alternatives. A further important difference lay in the increase rather than in a decrease in the percentage at the fourth alternative step for the first significant letter. Apparently observers who were unsuccessful did not perceive the relationships involved in the maze as readily or with the degree of clarity as did individuals who were successful.

It was noted from general observation and the reports of observers that many persons who solved the maze grasped the relationships readily, and that such individuals proceeded without error to the goal. Such rapid progress was not made by all observers. Certain persons not solving the maze appeared to respond randomly, trusting to luck that they might arrive at the goal. Others were totally lost with respect to the meaning or significance of their choices. Still others constructed elaborate schemes and persisted in these. These persons appeared greatly surprised at the simplicity of the correct scheme at the conclusion.

Interpretations of Results.—One would expect that if individuals responded to the identical elements of this maze rather than to relationships, that the percentages at the fourth alternatives for each of the significant letters would have either increased as a result of the influence of repetition, recency, and effect, or at least they should have been as great as the percentages found for the preceding three alternatives. If individuals responded to the elements of this problem as they perceived these elements in relation to the scheme of the maze, and to the further elements of progress and failure, then one

might have expected results as here found. It is apparent that the significant letters were perceived as having the attribute of correctness for the first three alternatives, and were identified with incorrectness at each fourth alternative. This differentiation had to emerge, and it appears to have developed more rapidly with some observers than with others, but in all instances it is identified with learning itself. When the meaning of these significant letters did not differentiate with respect to their order and position, learning was not successful as far as reaching the solution to the problem was concerned. Judging from the results for individuals who did not solve the problem, it appears that a differentiation in meaning or the perception of relations occurred to a significant degree. It is probable, however, that such relations were perceived less readily and failed to be further unified and identified with the general scheme of the maze as a whole, and that this was essential to the solution.

It would seem that the possible effects resulting from repetition, recency, satisfaction, or annoyance could not possibly have aided the learner in arriving at the solution of this problem, unless such effects operated in harmony with, and were dependent upon perceptual discrimination of relations with respect to the meaning of progress and failure as defined by the end toward which the observers were striving.

From the general pattern of the results for observers not solving the maze, one can justify the opinion that repetition, recency, and effect did not serve to prevent learning in these instances. The results for these observers do suggest that possibly chance factors in past training, intelligence, and motivation or interest may have played an important rôle. In any event, the evidence is clear that individuals who solved the maze were able to report the nature of the relationships and general scheme, while persons not solving the maze possessed only a fragmentary knowledge of these relations, or had no conception of them at all.

Conclusions

1. Meaning as a result of perceived relations was the factor possessing the greatest influence in the determination of choices of alternative letters in the mental maze constructed for this study.

2. Responses to identical elements varied significantly with arbitrarily designated changes from correctness to incorrectness at each fourth alternative step of the maze.

3. On the whole, there was no significant increase in the percentages from the first to the third respective alternatives for significant letters when such letters were correct, contrary to what one might expect from the supposed influence of repetition, recency, and satisfaction or annoyance.

4. Observers not solving the problem in general responded similarly although with less effectiveness than did observers who were successful.

5. Observers not solving the problem were unable to analyze the relations of the maze completely in the time allotment of one hour, as was noted from general observations and the reports of these observers.

6. Certain observers solving the problem grasped the total relationships of the maze quickly and suddenly, while others progressed more slowly.

7. Chance factors in training and experience, intelligence, motivation and interest are suggested as possible elements influencing the efficiency of learning in this problem.

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THE EFFECT OF VARIOUS STIMULI ON THE BASAL METABOLIC RATE, THE BLOOD PRESSURE AND THE GALVANIC REFLEX IN MAN

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Introduction

C. W. Darrow [2] concludes, among other things, in his study of 'Electrical and Circulatory Responses to Brief Sensory and Ideational Stimuli' that "there are two physiologically as well as psychologically distinguishable processes which have frequently been designated as 'emotion,' namely, the immediate reflex response to sensory excitation, and the response mediated by associative processes or ideas aroused by the stimulus." Again, Darrow says, that "A comparison of the immediate reflex effect of stimuli with the effect mediated by associative or ideational processes, shows that the former is relatively more effective in producing changes in peripheral mechanisms, such as those of vasoconstriction, perspiration or galvanic skin reflex, while the latter is relatively more effective in increasing cardiac activity as indicated by blood pressure or pulse rate." He further states that under the conditions of his experiments, "blood pressure differentiates better between 'disturbing' and 'indifferent' ideational stimuli than does the

[1] The writers are indebted to the Administration and to Drs. F. H. Krecker and James P. Porter, Chairmen of the Biology and Psychology Departments, respectively, in Ohio University for apparatus with which to carry out this research.

[2] Darrow, C. W., Electrical and circulatory responses to brief sensory and ideational stimuli, *Jour. of Exper. Psychol.*, 1929, **12**, 267–300. See also Darrow's review of literature of differences in physiological reactions to sensory and ideational stimuli in the *Psychol. Bull.*, 1929, **26**, 185–201.

galvanic skin reflex." Darrow's study bears directly upon our study since we used the same stimuli he used. Further, both investigations involve blood pressure and galvanic response measurements but in addition our study involves the measurement of metabolic rates.

Charlotte Purdy [3] *et al.* in a study of the 'Relationship Between Electrical Differences of Potential in the Skin and Normal Basal Metabolism,' present data showing that "higher basal metabolic rates are accompanied by lower differences of potential and vice versa." These data were obtained under normal conditions without the presence of exciting stimuli.

Edith Totten [4] has measured 'Oxygen Consumption During Emotional Stimulation,' and reports conflicting results. She performed fourteen experiments. In seven of these no change in rate of O_2 consumption following stimulation was obtained. In six experiments increases in the rate of O_2 consumption following stimulation was obtained. These increases ranged from 4.9 to 25.0 percent. According to the author the magnitude of these increases would indicate that they were due to changes other than internal ones. Additional experiments seemed to show that these increases in O_2 consumption were not due to increased tension or activity of the skeletal muscles, including the respiratory muscles.

Carney Landis [5] in the V of a series of 'Studies of Emotional Reactions' endeavored to bring about an 'emotional upset' and to record as fully as possible the physiological reactions of the subject during the 'emotional upset.' The reactions measured were blood pressure, metabolism rate, gastric contractions, rectal contractions and thoracic and abdominal respiratory movements. The 'emotional upset,' the effect of which he endeavored to measure, was brought about by a strong faradic current. Landis [6] does not give

[3] Purdy, C., Johnson, A. L., and Sheard, C., The relationship between electrical differences of potential in the skin and normal basal metabolism, *Science*, 1931, **73**, No. 1880, 46–49.

[4] Totten, Edith, Oxygen consumption during emotional stimulation, *Comp. Psychol. Monog.*, 1925, **3**, No. 13.

[5] Landis, Carney, Studies of emotional reactions. V. Severe emotional upset, *Jour. of Comp. Psychol.*, 1926, **6**, No. 3, 221–242; see also Landis, Studies in emotional reactions. IV. Metabolic rate, *Amer. Jour. of Physiol.*, 1925, **74**, 188–203.

[6] Landis, however, gives complete figures in Study IV, metabolic rate, *Amer. Jour. of Physiology*, 1925, **74**, 188–203.

complete figures of his determinations of metabolic rate and blood pressure in paper V but claims a decrease in metabolic rate during and following this 'emotional upset.'

The figures given, however, show for his three subjects an increase of forty-eight, twenty-nine and sixteen percent for each subject respectively in metabolic rate just previous to the stimulation. What can this mean but that during the 'anticipatory' period there was an emotional effect perhaps due to attitude or thought processes about the coming stimulus? These figures are very comparable to our data resulting from verbal stimulation. During his 'emotional upset,' or the electrical stimulation period, he claims a fall to thirty four, two, and fourteen percent above normal; but this fall is from a previous higher rate evidently due to 'anticipation.' Moreover, Landis claims the subjects continue to fall in basal metabolic rate after the period of stimulation. In fact the fall s to −6, −4 and −10, for the three subjects, respectively. This is, of course, what one would expect, namely, that the subject would return to normal. Yet, Landis does not include the 'anticipatory period' as a part of the faradic stimulation. Therefore, according to his own data, Landis has not determined the effect of faradic stimulation on basal metabolic rate.

Landis and Hunt,[7] in a more recent publication, while working with psychopathic patients, have the following to say: "The relation between BMR and emotional reactions . . . is particularly interesting since positive emotional reactions, which were obtained with the fifteen patients on which we had BMR's, occurred in all but one instance with the lowest metabolic rates."

Darrow and Heath's [8] critical experiments bear directly upon the problem we have attacked. In the 'Studies in the Dynamics of Behavior' they report twenty-one measures of physiological reaction in relation to (1) Neurotic Inventory, (2) Northwestern University Introversion-Extroversion Test,

[7] Landis, C., and Hunt, Wm. A., Adrenalin and emotion, *Psychol. Review*, 1932, 39, No. 5, 467–485.

[8] Lashley, K. S., Stone, C. P., Darrow, C. W., et al, *Studies in the Dynamics of Behavior*, University of Chicago Press, 1932, p. 239.

(3) Histograms showing the distribution of answers to representative questions, (4) Composite scores obtained by compiling answers to groups of similar items from the Neurotic Inventory, etc. Among other things, they report "blood-pressure response is disproportionately great after verbal (ideational?) stimuli." [9] Further, that the combination of large galvanic with small blood pressure changes after conditioned and non-conditioned stimuli was found most frequently in individuals whose answers to questionnaires suggest a comfortable adjustment to life situations. The fact is further revealed that the galvanic reactions are not the best indicator of emotions but that they may be correlated with health and extrovert tendencies.

The above discussion leads us to our problem, namely, to see if ideational, as well as sensory, stimuli (as used by Darrow) influence metabolic rate, blood pressure and the galvanic skin response. This problem is particularly pertinent in the light of Totten's conflicting results and since Landis' work shows increase in metabolic rate during the 'anticipatory period' but an actual fall of metabolism during the application of the faradic stimulus, it was felt that perhaps under carefully controlled experimental conditions more light might be thrown upon the effect of various stimuli upon metabolic rate, blood pressure and galvanic skin response.

Since unequivocal results, under rigidly controlled conditions, on basal metabolic rate during emotion-arousing conditions have not been reported, it is the investigators' aim in the present study to make synchronized measurements of three physiological processes, metabolic rate, blood pressure and galvanic skin response in male subjects under the influence of stimulating conditions. Such questions arise as: Do various stimuli (ideational and sensory) increase or decrease basal

[9] It is the present writers' opinion in the study of adult emotions the rôle of perception and meaning of stimuli which have emotional value will have to be examined. Sudden 'starts' with accompanying physiological changes may follow an unexpected slap on the shoulder by a friend, but the peripheral reactions soon subside. But let the individual slapped turn and perceive an enemy taking that liberty then there may be aroused deep seated physiological and psychological reactions which may persist over a period of time.

rate? Are these changes, if present, accompanied by blood pressure changes and galvanic response changes? The practical aspect of this problem involves various degrees of energy consumption under normal and exciting conditions. The results reported in this paper deal chiefly with basal rate and blood pressure changes. Since the photographic apparatus of the galvanometer failed us on some of the subjects, that data will have to be re-checked. The subjects' records that photographed well, however, reveal the 'skin response' clearly in its relation to blood pressure changes.

Technique and Procedure

In attempting to measure the influence of various stimuli on metabolic rate, blood pressure and galvanic skin response in human male subjects the following technique was used. To measure basal metabolism, a Benedict-Roth apparatus[10] was used.

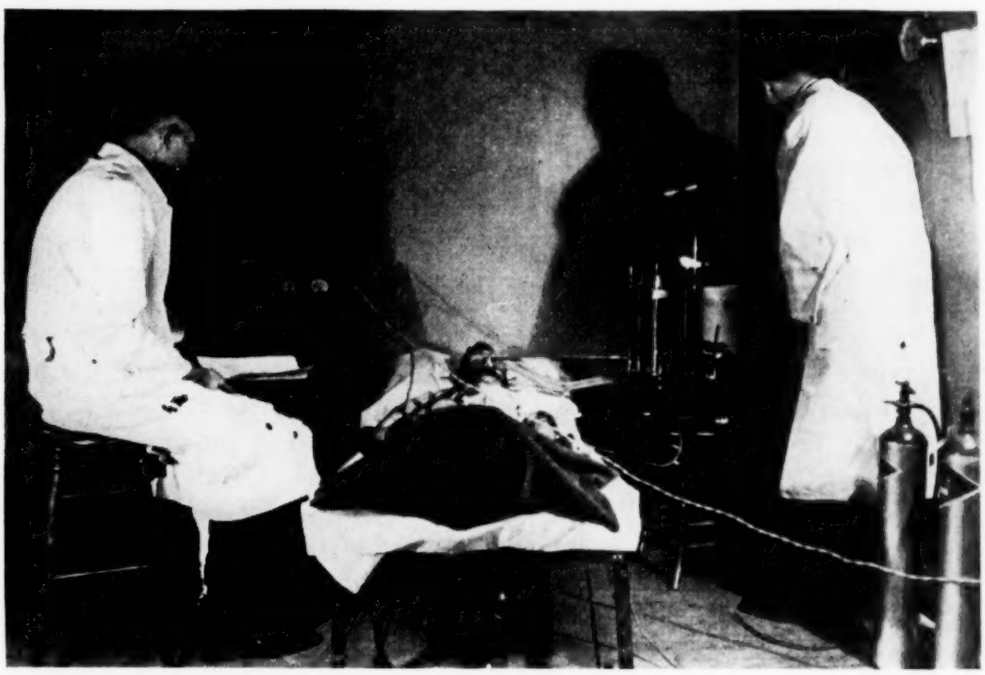

Fig. 1. Photograph of apparatus attached and operating.

(See Fig. 1 for set-up.) This apparatus was selected because it combined simplicity of operation and a reasonable degree of scientific accuracy.

To measure blood pressure changes a Tycos sphygmomanometer cuff was placed around the upper left arm of the subject. This apparatus was then attached through a reducing capsule[11] to a tambour and marker. The marker was a light rigid bamboo

[10] Manufactured by Warren E. Collins, Boston, Mass.

[11] Dunlap's 'Reducing capsule' gave very satisfactory results in this set-up.

lever adjusted so as to record the pulse waves on the kymograph drum of the metabolic apparatus. The pulse curve was sychronized with the respiratory tracings on the metabolism apparatus drum. Tests showed this type of recording lever to be quite adequate to give relative changes in the blood pressure, but not to record the absolute blood pressure.[12] The absolute blood pressure was taken by a sphygmomanometer reading (auscultatory method) just before and just after each test. All objective blood pressure records were taken as shown in Figs. 2 and 3. Waves of increased pressure are recorded by this apparatus following the stimulation of the subject. Further, changes accompanying movements of the subject would be recorded. The experimenters, however, took care to note any movements of the subjects and cautioned subjects against making them. Movements of subject's arms, if they did occur, gave quite different effects than the stimuli.

For changes in the skin response the Hathaway galvanometer with a Ruckmick photographic attachment was used. Metal electrodes were attached to the palm of the right hand.

As was stated above, young male subjects (College students) between the ages of nineteen and twenty-nine years, were used in these tests.

The normal basal metabolism was determined first by running four double tests on each subject. (See Table 1 for exceptions.) As a rule one double test was given each subject a week; the exception was that two subjects were given two double tests a week. The usual precautions as to the proper condition of the subject [13] were observed in taking these tests. The apparatus was tested at regular intervals for leaks and its efficiency in removing CO_2 from the respired air. The four double tests under basal condition were followed by four more double tests; (see Table 1 for exceptions) the latter constituted a control series while the former were considered as basal for the subject. During the control series the electrodes were attached to the subject, the sphygmomanometer cuff was attached to his arm and the pressure in the cuff adjusted to the experimental level of the 80 mm Hg. No stimuli were given during the control series. As will be shown later, the effect of the apparatus attached and operating on the basal metabolism was surprisingly small.

The last double test made on each subject included the experimental test. A basal was taken with the apparatus attached and operating as described under the control series above. This was followed by the final test during which time the subject was subjected to the stimulating conditions as subsequently shown. All tests were made between 6:30 and 9:00 A.M. under normal basal conditions with, of course, the exception of the experimental procedure as for columns 7 and 8 in Table 1.

Since Darrow claims to have distinguished different effects on the individual with different types of stimuli, part of his selection and classification of stimuli was used to produce emotional changes.

The order in which the above stimuli were given was varied with the different subjects to determine the effect, if any, of sequence. The stimuli were given during the usual ten minute period of the metabolism test, as nearly as possible at half-minute intervals. One experimenter gave the stimuli while the other watched the subject and apparatus. The subjects were cautioned just before the presentation of stimuli to

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[12] For a discussion of blood pressure techniques see Matthew Chappell, A comparison of blood-pressure methods in the *Jour. of Genetic Psychology*, 1931, **39**, No. 3.

[13] No eating after 8 P.M. the night before; at least 8 hours sleep, least activity possible in the morning and quiet during tests; pulse taken frequently for at least 30 min. before tests were begun while subject was lying down.

LIST OF STIMULI

Indifferent Stimuli	*Sensory Stimuli*	*Ideational Stimuli*
Box	Dropped chair	Constipation
Chair	Scraped electric fan	Masturbation
Door	Snapped fingers	Menstruation
Pencil	Touched forehead	Petting
	Clapped hands	Pregnancy
		Sex
		Success
		Money

make no movements at any time during the presentation and they were also asked to think about the verbal stimuli when given. Many of the stimuli were given in the form of phrased questions such as "Do you wish you were a success?," "Masturbation, what do you know about masturbation?" While answers were not called for, it will readily be seen that these stimuli were put in such a way as to arouse thought processes. The subjects had no clue as to when the experimental test would come—hence anticipitation if any, was of very short duration. If present at all it could have lasted only between the time of the instructions, "I am going to give you some words," and the actual presentation of the words. Movements of the body gave much sharper changes in the blood pressure curve than the stimuli elicited and could, as far as we could observe, be quite easily detected. (See sharp rise in control curve in Fig. 3.)

RESULTS AND DISCUSSION

Table 1 shows all the results of basal metabolism tests made on each subject. These tests were made in sets of two (*a* and *b*) on different mornings. Eight basal tests, under normal conditions, were made on five subjects, seven on one, and five on one. Seven subjects in all were used. These tests extended over a period of several weeks.

Thus in Column 6, Subject I (G.R.E.) showed a minimal basal of −22.0 percent and a maximum of −5.1 percent on different days. In Column 7 the next series of tests are shown. One of the two consecutive tests on a given morning was made under normal conditions and the other (either following or preceding) was made with all apparatus attached and operating but with no stimuli given. The latter tests are starred. These starred tests act as a control for the experimental test which followed. Example: Subject I (G.R.E.) shows a normal basal in the first test of the control series to be −6.5 percent (*a*) and test (*b*), with the apparatus attached and operating, to be −5.9 percent. The greatest variation from the basal rate in this subject, due to the apparatus being at-

TABLE 1

SHOWING RESULTS OF ALL METABOLISM TESTS MADE ON ALL SUBJECTS; ALSO BLOOD PRESSURE AND PULSE PRESSURE READINGS

Subject	Age	Body Wt. lbs.	Ht. In	S.A. Sq. M	Basal Rate Percent Variation from Normal (Aub-Dubois)				Control Series Percent Variation from Normal (Aub-Dubois)				Experimental Tests Percent Variation from Normal (Aub-Dubois)	Absolute Blood Pressure mm Hg	Pulse Pressure mm Hg
Col. 1	Col. 2	Col. 3	Col. 4	Col. 5	Col. 6				Col. 7				Col. 8	Col. 9	Col. 10
					1	2	3	4	1	2	3	4			
														Sys. Dias.	
I (G.R.E.)	25	160	71	1.91	*a.* − 2.20 *b.* − 2.20	− 6.5 − 9.3	−11.0 −10.7	− 5.1 − 8.4	*a.* − 6.5 *b.* − 5.9*	−10.0 − 9.8*	− 4.7* −11.0	− 0.04* − 4.0	− 5.3* +29.0**	Before 106—78 After 112—78	28 34
II (C.B.)	21	133	66	1.69	*a.* − 5.4 *b.* − 8.0	− 9.03 −11.7	− 6.6 − 2.6	− 5.5 − 3.2	*a.* − 6.5 *b.* − 7.9*	− 6.8 − 8.8*	− 3.0* − 1.6	− 7.9* − 7.9	− 7.6 + 9.1**	Before 112—78 After 126—74	34 52
III (P.S.S.)	23	146	68½	1.80	*a.* −17.0 *b.* −12.0	−10.8 + 2.3	− 2.0 − 6.0	− 9.9 − 3.7	*a.* −11.3 *b.* −11.3*	+ 1.7 − 8.7*	− 3.3* − 3.3	− 8.3* − 3.3	+ 1.7* +11.00**	Before 120—74 After 126—78	46 48
IV (O.T.)	21	126	68½	1.69	*a.* − 6.5 *b.* −16.0	−14.0 −16.0	−27.0 −14.9	− 9.5 −14.9	*a.* −10.0 *b.* −18.1*	−13.0 −18.0*	− 1.4* + 3.4	− 7.0* − 9.0	− 5.9* −15.0**	Before 116—76 After 116—70	40 46
V (R.M.)	29	205	65	2.00	*a.* + 5.0 *b.* − 3.8	− 4.3	+ 1.5 + 0.36		*a.* + 8.7* *b.* − 4.3				− 8.1* +29.9**	Before 130—80 After	50
VI (B.H.)	19	142½	67½	1.76	*a.* −11.0 *b.* − 8.6	−10.6 − 9.4	− 9.6 −15.0	−12.0 −18.0	*a.* + 1.7* *b.* + 7.5	−13.0	−16.0 −12.0*	−16.0 −12.0*	−16.0* − 1.3**	Before 104—78 After 104—78	26 26
VII (J.B.)	23	137½	67½	1.73	*a.* − 3.2 *b.* − 3.2	−15.0 −14.0	−15.0 −11.0	−10.8	*a.* − 7.0 *b.* − 5.6*	− 4.2 − 6.0*	− 5.6* − 9.4	−15.0* −14.0	−14.0* − 1.8**	Before 110—66 After 110—70	44 40

tached and operating, was during the third tests. The first test with the apparatus in this set is −4.7 percent, while the second, without the apparatus, is —11.0 percent.

As was said before the results in Column 7 serve as a control series for Column 8. The tests in Column 8 without the stimuli, but with apparatus attached and working, are marked with one star; the tests made under the influence of the stimuli are marked with two stars. Thus Subject I (G.R.E.) test (*a*) with apparatus adjusted and operating gives a metabolism rate of −5.3 percent; but test (*b*) made under the influence of stimuli as +29.0 percent.[14]

Simultaneously with the measure of metabolic rate, blood pressure changes were recorded as are shown in Figs. 2 and 3. The experimenters do not claim measures of absolute but only relative blood pressure changes by this recording device. The absolute readings were taken just before and just after the tests (auscultatory method); these results are recorded in Column 9. An examination of Column 9 shows both systolic and diastolic readings. Take Subject I (G.R.E.) again: before the test his systolic pressure was 106 mm Hg. while after the test his systolic was 112; before test his diastolic was 78, after the test, 78 also. Pulse pressures are shown in Column 10.

In Table 2 the results shown in Table 1 are further analyzed and condensed. In Column 1 all basal tests made under normal conditions, without the influence of apparatus are averaged for each subject. Column 2 shows (control series) averages of all tests for each subject made with apparatus attached and operating but without stimuli being applied. Column 3 (Exp. tests) shows test rates with apparatus attached and stimuli being applied.

Column 4 shows variation in basal rate (percent of variation from normal) between Columns 1 and 3, namely, between metabolism under basal conditions and metabolism under stimulating conditions.

Column 5 shows the variation between 2 and 3, namely,

[14] Although some may not agree, the present experimenters feel that since the trend of the results, with one exception, points in one direction the results have been given in percents only—hence we have not submitted the data to statistical procedure.

TABLE 2

SUMMARIZING ALL TESTS ON EACH SUBJECT

Subject	Basal Rate % Variation from Normal (Aub-Dubois), Avg. 12 Tests Col. 1	Control Series % Variation from Normal (Aub-Dubois), Avg. 4 Tests Col. 2	Experimental Tests % Variation from Normal (Aub-Dubois) Col. 3	Difference between Col. 1 and Col. 3 Col. 4	Difference between Col. 2 and Col. 3 Col. 5	Blood Pressure Change during Exp. Test, Sys. Dias. Col. 6
I (G.R.E.)	−10.54	− 5.11	+29.00	+39.54	+34.11	+ 6 o
II (C.B.)	− 6.23	− 6.50	+ 9.10	+15.33	+15.60	+14 −4
III (P.S.S.)	− 6.27	− 7.90	+11.00	+17.27	+18.90	+ 6 +4
IV (O.T.)	−12.28	−11.12	−15.00	− 2.72	− 3.88	o −6
V (R.M.)	− 0.92	+ 0.30	+29.90	+30.82	+29.60	— —
VI (B.H.)	−10.98	− 7.43	− 1.30	+ 9.68	+ 6.13	o o
VII (J.B.)	− 9.70	− 8.05	− 1.80	+ 7.90	+ 6.25	o +4
Total avg.	− 8.13	− 6.68	+ 8.70	+16.83	+15.38	

Explanation. Column 1 shows avg. of all normal basal tests on each subject. Column 2 shows avg. of all tests made on each subject with apparatus attached. Column 3 shows the rate of metabolism during the stimulation. Column 4 shows the difference in metabolism rate (percent variation from normal) between avg. of normal basal rate and the rate under stimulating conditions, for each subject. Column 5 shows the difference in the metabolism rate (percent variation from normal) between avg. of control series and the rate under stimulating conditions, for each subject. Column 6 shows the blood pressure change during the experimental test, from absolute blood pressure readings taken just before and just after the test.

between metabolism rate with apparatus attached and metabolism rate under stimulating conditions. An example will illustrate: Subject 1 (G.R.E.) has an average metabolism rate under basal conditions of −10.54 percent; under conditions with apparatus attached and operating of −5.11 percent; a difference of 5.43 percent rise. This rise is not significant since greater variations occurr in some subjects between two basal tests. The metabolic rate under stimulating conditions

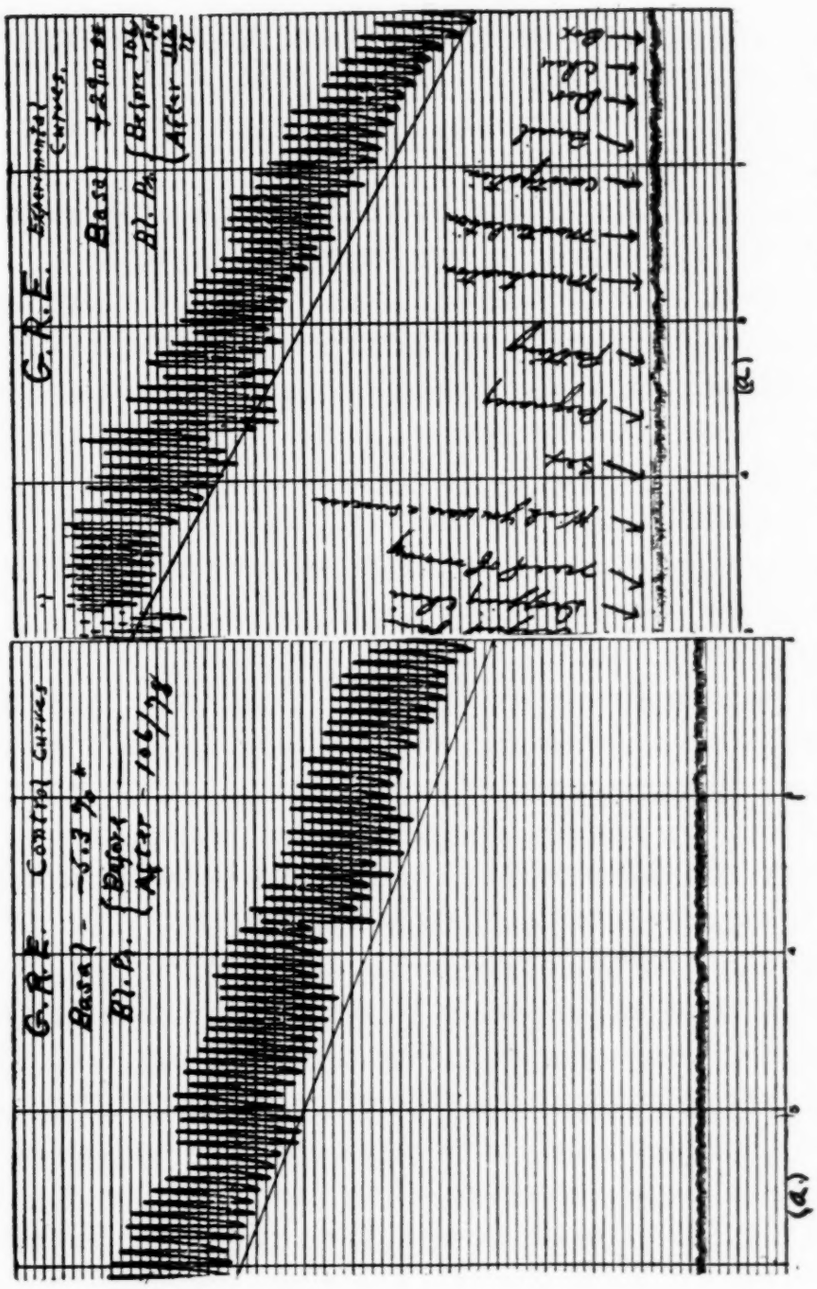

FIG. 2. Showing the Respiratory Curve and the O_2 Consumption Line above and Blood Pressure Curve below. The Blood Pressure Curve shows small rapid pulse waves, longer respiratory waves, and still longer waves during the stimulatory period. The stimuli used are shown with an arrow pointing to the time when they were given.

is +29.0 percent (Col. 3). This shows a rise of 39.54 percent over the average metabolism rate under basal conditions for this subject, and a rise over metabolism rate with apparatus attached and operating of 34.11 percent.

Figure 2 shows a section from the experimental respiratory and blood pressure curves of Subject I (G.R.E.); also the control curves of the same subject taken just previous to the stimulating test.

The blood pressure control curve shows the usual respiratory waves corresponding to the respiratory rate in the respiratory curve. Comparing this blood pressure curve with the blood pressure curve during the experimental stimulation, the increased amplitude and length of the waves in the latter are plainly seen. In the control curve (segment *a*)[15] the number of respirations is 12 and the number of respiratory waves in blood pressure curve is 12. In the experimental curves (segment *a*) there are 14 respirations but only five of the longer, higher waves in the blood pressure curve. The smaller respiratory waves lose somewhat their identity in these longer waves during stimulation. A further analysis of this subject's curve shows the verbal stimuli about 'Petting' and "do you wish you were a success" ? produce the greatest effect on blood pressure. The metabolism rate, as will be recalled for this subject in the control test is −5.3 percent; while for the experimental test, it is +29.0 percent. The difference in the slope of the curve is obvious. While the curve shows definite relative changes in blood pressure following the application of stimuli, the experimenters have not been able to find consistent, pronounced differential effects of the various stimuli ('sensory' and 'ideational') in any one or in all subjects. In general, however, there seems to be greater changes in blood pressure following the application of verbal (ideational) stimuli than for the so-called 'sensory' stimuli. These changes could not be due to arm or body movements for none such were observed in the experimental test. Arm movements give a very different effect (see control curve in Fig. 3).

[15] Each segment between the heavy perpendicular lines represents one minute interval of time.

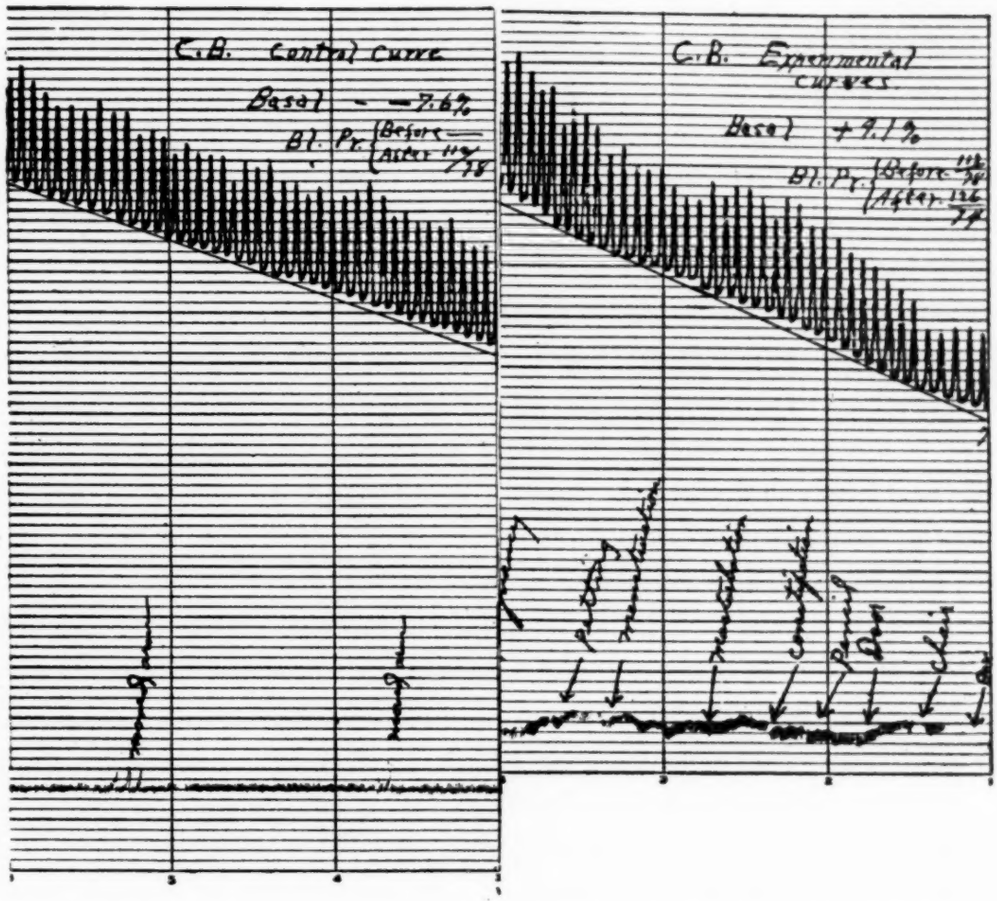

FIG. 3. Showing the Respiratory Curve and the O_2 Consumption Line above and the Blood Pressure Curve below.

Note: Only representative samples of graphs are published; the other graphs are on file in the Biology Laboratory, Ohio University.

Figure 3 shows curves of Subject II (C.B.). The metabolic rate in the control and experimental tests are — 7.6 percent and + 9.1 percent, respectively. The blood pressure control curve here shows the usual respiratory waves. The experimental blood pressure curve of this subject, however, shows waves of larger and longer duration than the control curve. These longer waves are very pronounced in this subject. These waves seem to be general during stimulation, rather than specific following definite stimuli. The verbal stimuli 'constipation' and 'menstruation' are followed by a more pronounced blood pressure changes in this subject. Incidentally, it was noted in some of the subjects that the initial stimulus, although of an 'indifferent' nature, was

followed by a rather prominent change in blood pressure. In addition the pulse pressure as shown by amplitude of pulse wave is decidedly greater in the experimental curves than in the control curves; this is strikingly shown in Fig. 3.

Since the rate of metabolism was measured by determining the O_2 consumption for a 10 minute period, and since the various stimuli were given during one of these periods, it is of course impossible to determine the effect of any one stimulus or group of the stimuli on the rate of metabolism; only the total effect was obtained. This effect, however, is marked and is shown to be beyond any possible variation in normal rates. Since 12 normal basals were used as controls and 5 basals with apparatus attached and running before the stimulation test, ample check was provided.

In the seven subjects, five had a metabolism rate during stimulation much higher than in any control test; one (B.H.) had a rate during stimulation which was lower than some of the controls, but (see Col. 8, Table 1) was 14.7 percent higher than the control rate for the same day taken just previously to the stimulation; one subject (O.T.), proved the only exception to the general result in that his rate of metabolism was lower during the stimulation than under control conditions.[16]

In Columns 4 and 5, Table 2, the effect of the stimuli on the rate of metabolism is summarized. With the exception of one subject (O.T.) the increases over the normal basal rates, allowing for effect of apparatus, are all marked and vary from 6.13 percent to 34.11 percent and show an average increase in all subjects of 15.38 percent. This seems to demonstrate that verbal and sensory stimuli are followed by a distinct rise of metabolism rate above the rate taken under basal and control conditions.

The stimuli produced irregular waves in the blood pressure curves of greater height and duration than the respiratory waves.[17] These were more marked in some than in others

[16] Subject IV (O.T.) may have been more sophistocated since he had assisted in previous experimental work in psychology and in physiology. It is interesting to note Landis' subjects were also sophisticated in this type of experimental work.

[17] These waves remind one of the Traube-Hering waves. They may be due to spleenic action. J. Barcroft and his co-workers have shown the spleen to be involved in

but were present in all. This seemed to be a general effect of the stimuli on the blood pressure, causing irregularity in the curves independently of the type of stimulus, although certain stimuli were followed by more marked rise than others. The difference in the amplitude of the pulse wave in the different blood pressure curves is due to differences in the pulse pressure.

In Fig. 3 the pulse wave is of much greater amplitude during the stimulation period, but the pulse pressure at the end was found to be 18 mm Hg higher than at the beginning of the control test. (See Table 1, Subject C.B., Col. 10.)

Summary

1. The attachment and operation of the apparatus caused very little rise in metabolism rate over the basal rate.

2. Sensory and ideational stimuli have been shown in all subjects but one to be followed by a definite rise in metabolic rate.

3. This technique failed to get results which show pronounced differential effects between the so-called sensory and ideational stimuli on metabolic rate; although the blood pressure curves show some differential effect in favor of ideational stimuli.

4. Irregular waves of greater amplitude and longer duration than the respiratory waves were obtained in the blood pressure curves during the stimulation. Some of these waves followed specific stimuli of both ideational and sensory nature.

5. Measures of absolute blood pressure before and after stimulation showed a rise in both systolic and diastolic in some subjects but in others very little change occurred; likewise the pulse pressure showed a marked rise in some but not in others.

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emotions. See Barcroft, Joseph, "Some effects of emotion on the volume of the spleen." *Jour. of Physiol.*, **68** (4); 375–382, 1930. See further work of Barcroft on Traube-Hering waves.

CORRELATION OF ANIMAL AND VEGETATIVE REACTIONS (EXPERIMENTAL DATA)

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The conception of the reflex as a strictly isolated functional unit must be abandoned as it has been proved that together with the local reaction to a local stimulus the whole organism becomes involved in the responding action. Every specific reaction to a definite local action is accompanied by a general reaction. For a proper understanding of the peculiarities of the nervous process it is necessary to study the responsive changes and their correlation in all the sections of the nervous system. The above statement serves as a basis in the construction of the reflexological method of investigation in the laboratory of Prof. Miassicheff of the Bechtereff State Institute. The general reactions, the galvanic and the respiratory are recorded side by side with the local motor reactions responding to the electrodermal stimuli. The nervous process under study is treated as a result of the dynamic correlations within one system.

A single peculiarity, such as the absence or the retardation of the formation of the associated motor-reflex cannot serve as a basis for the diagnosis of the type of the nervous system. Difficulties in the formation of the associated motor-reflex may take place in both cases, in the presence of marked—as well as in that of weak vegetative reactions. In both instances one and the same property taken as an element may appear to be uniform, but taken as a whole they will be found to differ materially. In the first case we are inclined to see the inhibited type, while in the second case it is the torpid type of nervous system.

[1] The author expresses her thanks to Prof. V. V. Miassicheff for the suggestion of the theme, to E. P. Feoktistova for friendly advice, and also to the principals of the school, N. D. Vasilenko and D. I. Safonoff, for assistance at their school during the experiment.

The present investigation was conducted by two motor reflexological methods: the electro-dermal and the 'grabbing' (Cheranovsky's method). During the experiments by the two methods, the grabbing and the electro-dermal, the general reactions were recorded. The experiments were conducted in the following way: the subject was placed before the reflexological screen, the reactions of the hand were recorded on the drum of the kimograph by aerial transmission. The respiration was also recorded on the same drum by aerial transmission from the pneumograph of Lehmann's type which was in contact with the xiphoid process of the sternum of the subject. The strength of the galvanic reflex was registered simultaneously. For this purpose the subject was included into the chain known as the Muller system, which consists of an outside source of electro-dynamic power, an accumulator, a potenciometer and a mirror galvanometer of Hartmann and Brown's type, having a sensibility of 10^{-8} amp. The strength of the galvanic reaction was determined in millimeters according to the change of position of the light image which appeared on the scale placed at a distance of one meter from the galvanometer. The reflexological experiment by the electro-dermal method was conducted by training of the associated motor reflex to a color stimulus as well as its differentiation.

The experiment consisted of one session only which included 31 stimuli given in the following order:

5 stimulations each consisting of 5 (an associate and electric current).

10 stimulations as follows: 4 (an associate and electric current), 2 associated, 4 differentiated.

10 stimulations as follows: 3 (associate and electric current), 3 associated, 4 differentiated.

6 stimulations as follows: 1 (associate and electric current), 2 associated, 3 differentiated.

The applied method did not completely determine the so called 'reflexological' type as regards motor reactions, but showed some tendencies concerning the production and differentiation of an associated motor reflex. This was sufficient, in the present case, for the investigation of the correlation between the reactions of the animal and vegetative systems.

The experiment by the 'grabbing' method, consisted also of training of the associated motor reflexes (a grabbing motion of the arm and the snatching of a ball) to a color stimulus,

and the establishing of a differentiation. This method of training of the associated motor reflexes was preceded by an instruction of the subject to snatch the balls and by a demonstration of this action both by the experimenter and by the subject.

The conditions of these two experiments have some similar moments on one hand and marked differences on the other. The similarity consisted in the fact that in both cases there was conducted the training and the differentiation of the associated motor reflexes to color stimuli. The specific peculiarities of the non-conditioned reflexogenic stimuli were in both cases very different. While in the electro-dermal method the pain stimulus is doubtless of a reflexogenic nature, one can hardly see in the touching of the ball, in the grabbing method, a non-conditioned stimulus causing the grabbing motion of the hand. This was confirmed by the author of this method, Dr. Cheranovsky, who in his experiment had to introduce a preliminary instruction of the subject and demonstration of the action in order to cause a grabbing motion at the touching of the ball. It is evident that while in the electro-dermal method we have to deal with a nonconditioned pain stimulus in response to which the formation and differentiation of the associated reflex takes place, in the 'grabbing' method we find a much more complicated process due to the inclusion of instruction and demonstration. Besides the above mentioned difference there must also be pointed out the difference in the physiological value of the electro-dermal and the tactile stimuli. This is of particular importance for the tabulation of the accompanying vegetative reactions. The electric current is a strong pain stimulus, the touching of the ball a weak one, or rather an indifferent one. Another peculiarity of the 'grabbing' method to be mentioned is that this method is constructed in such a way that the associated stimulus is always (including also the cases where associated-motor reflexes are formed) supported by the non-conditioned stimulus—the touching of the ball. Therefore, in analyzing the changes occuring not only in the motor system (where this is possible) but also in the other systems, the conditions of the experiment make a record of these changes impossible owing to the absence of an isolated associated stimulus. Thus when applying the above mentioned method we get no data which would enable us to judge either of the strength of the galvanic reflex in response to the action of the associated stimulus (non-supported) or of the presence of the so called psycho-galvanic or associated galvanic reflex. This peculiarity of the method becomes also apparent during the analysis of the respiratory reactions.

Taking in consideration the peculiarities applied at the above methods of investigation and with the point in view of obtaining comparative data, we have subjected the working out of the material to a definite plan and arrangement.

The material for the work was provided by the results of an investigation of 30 (the experiment was started with 40 children, but since the investigation of 10 of them was not completed for reasons not depending upon the investigator, the data concerning these 10 children were not taken into consideration) subjects; boys from 14 to 15 years of age, pupils of the 203rd school for problem children. The sub-

jects underwent experiments both by the first and the second methods, with an interval from 2 to 3 months. It was interesting to ascertain the galvanic reactivity in the course of the same experiment, but during its different moments; for instance, galvanic reactivity by the electro-dermal method with the associated stimulus, both supported and non-supported by the electric current, and during analogous moments of different experiments (for example, the strength of the galvanic reflex to an associated color stimulus under conditions of both, the electro-dermal and the grabbing methods etc.).

For the characterization of the galvanic reactions the average figures were calculated for different moments of the experiment for each subject separately. Thus were calculated the average figures of the galvanic reflex to a current, to the associated and to the differentiated stimuli. The comparison of the results obtained from all the subjects by the electro-dermal method showed that: *The strength of the galvanic reflex to a current varied in different subjects considerably from 11 to 189.* It remains uncertain whether there exists a correlation between the strength of the galvanic reflex to the current and the sensibility for it. *In comparing the strength of the galvanic reflex to currents with the maximum limit of the electro-dermal sensibility we get data showing frequently an inverse correlation between them; i.e.* a weak galvanic reflex corresponding with a greater resistance to electric currents.

TABLE 1

Subjects	Average Strength of the Galvanic Reflex to Currents	Maximal Limit of the Electro-dermal Sensibility
K-in	12.1	90 v.
K-nov	11.0	100 v.
N-ew	28.8	120 v.
F-ov	18.0	90 v.

A strong galvanic reflex corresponds with a weak resistance to electric currents.

TABLE 2

M-kov	189.2	40 v.
Vev	140.8	20 v.
S-nov	157.0	30 v.
K-rev	159.0	40 v.
G-ev	121.7	50 v.
P-ov	101.3	40 v.
M-ov	139.0	30 v.

A direct correlation is observed comparatively rarely.

TABLE 3

G-ov	153.6	100 v.
M-ev	29.2	30 v.

The average strength of the galvanic reflex under the conditions of the experiment by the electro-dermal method for all the subjects is:

TABLE 4

Stimulus	Minimum and Maximum Limits of Strength of Galvanic Reflex	M (arithmetical mean)	σ (sigma)	m (error of arithmetical mean)
Current	11.0–189.2	79.71	49.40	9.02
Associated (color)	3.8–122.0	34.71	25.84	4.72
Differentiated (color)	3.7–110.0	20.30	24.75	4.52

From these data we can draw the following conclusions:

1. *The strength of the galvanic reflex under conditions of the conducted experiment varied widely in different subjects.*

2. *The strength of the galvanic reflex to currents—as it was to be expected—exceeded the strength of the galvanic reflex to color stimuli to a considerable extent. This could be explained by the greater strength of the first stimulus as compared with the second. The greatest strength of galvanic reflexes, as the investigations of Feoktistova have shown, was obtained in response to an action of a strong external stimuli, such as the sound of an automobile horn and a double prick with the estesiometer into the dorsal surface of the hand.*

3. *Comparing the strength of the galvanic reflex to an associated stimulus with that of the reflex to a differentiated one, there was noticed a considerable increase of its strength to the associated stimulus over the differentiated one. The above mentioned increase in strength of the galvanic reflex in response to an associated stimulus indicates that in the general responding reaction to an associated stimulus a momentum of an associated reflectory nature takes place.*

Concerning the psycho-galvanic (or associated-galvanic) reflex which was noted by Prof. Miasishchev in a marked increase of the strength of the galvanic reflex to an associated stimulus, reaching the strength of the galvanic reflex to electric currents,

and which could be frequently trained during the first seance, it was not confirmed on the author's material. The psycho-galvanic reflex was observed only in seven subjects and was of an extremely unstable nature, *i.e.* it was noted only once or twice during a session. However, as the results will show further, the galvanic reflex possesses the property of an associated nature. The comparative results concerning the strength of the galvanic reflex obtained under the conditions of the two applied methods, show us the mechanism of the formation of the galvanic reflex after the associated type; this will be shown below.

The average strength of the galvanic reflex in all the subjects (with the 'grabbing' method) is as follows:

TABLE 5

Stimulus	Minimum and Maximum Limits of Strength of Galvanic Reflex	M (mean)	σ (sigma)	m (error of arithmetical mean)
Associated and 'basic' (touching of the ball)	1.6–40.0	12.47	10.38	1.89
Differentiated	0.5–44.8	8.84	10.43	1.91

When comparing the strength of the galvanic reflex to an associated color stimulus by the 'grabbing' method with its strength by the electro-dermal method (it must be born in mind that in applying the 'grabbing' method, due to its peculiarity, the associated stimulus is always strengthened by the tactile) it becomes evident that it is considerably greater in the electro-dermal method:

TABLE 6

Method Applied	Minimum and Maximum Limits of Strength of Galvanic Reflex to Associated Stimulus	M (average strength of galvanic reflex to associated stimulus)	σ (sigma)	m (error of arithmetical mean)
'Grabbing'	1.6– 40.0	12.47	10.38	1.89
Electro-dermal	3.8–122.0	34.71	25.84	4.72

The observations are similar with differentiated stimulus:

TABLE 7

Method Applied	Minimum and Maximum Limits of Strength of Galvanic Reflex to Differentiated Stimulus	M (average strength of galvanic reflex to differentiated stimulus)	σ (sigma)	m (error)
'Grabbing'	0.5– 44.8	8.84	10.43	1.31
Electro-dermal	3.7–110.0	20.3	24.75	4.52

It was interesting to determine the correlation between the strength of the galvanic reflex (g.r.) in different moments of the same experiment and in analogous moments of different experiments.

TABLE 8

Experiment and Methods	Signs of Correlation, Strength of Galvanic Reflex to:	r (coefficient of correlation)	± m (error in coefficient)	Estimation Reliability
'Grabbing'	Associated and differentiated stimulus	0.73	0.09	Reliable
Electro-dermal	Electro-dermal and associated stimulus	0.96	0.014	Reliable
Electro-dermal	Electro-dermal and differentiated stimulus	0.91	0.03	Reliable
Electro-dermal	Associated and differentiated stimulus	0.98	0.007	Reliable
Both, *i.e.* 'grabbing' and electro-dermal methods	Associated and associated stimulus	0.067	0.18	Not reliable
	Differentiated and differentiated stimulus	0.036	0.19	Not reliable

It seems that in conditions of the same experiment by the 'grabbing' as well as by the electro-dermal methods (in the latter expressed more strongly) a considerable and certain correlation between the strength of the g.r. in different moments of the experiment (to different stimuli) is ascertained. However, no correlation has been ascertained between the strength of the g.r. to the same stimulus in conditions of different experiments.

Considering that there has not been established a correlation between the strength of the g.r. to an associated color stimulus in conditions of different experiments; that there is an increase of the strength of the g.r. to the associated color stimulus by the electro-dermal method over that of the 'grabbing' method; and that there is a considerable and certain correlation between the strength of the g.r. to an associated color and an electro-dermal stimuli, we can draw the following conclusions:

The increase of the strength of the g.r. to the associated and the differentiated stimuli in conditions of the experiment by the electro-dermal method shows that the physiologic action of only the color stimulus is excluded. We deal here with a mechanism of a different nature. Evidently the g.r. appears to be the responding reaction not only to the color stimulus, but to the whole

process in which the included current, a strong stimulating component, causes a considerable excitement.

In order to verify the expressed hypothesis, we started an additional experiment. The construction of the latter was based on these considerations: If the subject does not react to separate stimuli but reacts to the situation as a whole, there must be a difference in the strength of the g.r.; especially in that case when the subject, who had previously undergone an experiment by the electro-dermal method, is offered two situations. The first, in which the associated and the differentiated stimuli are combined in such a way that the possibility of obtaining a current is excluded. The subject is seated before the screen while his right hand rests on the table, instead of on the key. The second situation is in which the same color stimuli form such a combination, by which the obtaining of currents is made possible. The subject is instructed to hold his right hand on the key. In the first situation the g.r. must be of a lesser strength than in the second one, and their increase must be in correlation with the individual peculiarities of the subjects in relation to the galvanic reactivity. Three subjects were selected for the above described experiment. They were characterized during the preceding experiments with the following peculiarities of their galvanic reactivity:

TABLE 9

Subjects	Strength of the Galvanic Reflex					Maximal Limits of Electro-dermal Sensibility
	The 'Grabbing' Method to		Current	Electro-dermal Method		
	Associated Stimuli	Differentiated Stimuli		Associated Stimuli	Differentiated Stimuli	
I. K-lin.....	1.6	0.5	12.1	7.5	6.5	90 v.
II. K-lun....	10.2	2.9	42.6	39.0	34.7	60 v.
III. K-ev.....	19.8	8.5	159.0	62.7	63.3	40 v.

In the first subject there was noticed a comparatively small increase of the strength of the g.r. to associated and differentiated stimuli by the electro-dermal method as compared with the grabbing method; in the second subject the increase of the g.r. was more marked; in the third—it was extremely

high. It could be expected that each of the subjects should give various results, namely: in the first subject the difference in strength of the g.r. in the first and in the second parts of the experiment should be noticeable but little; in the second subject it must be considerable, and in the third—very pronounced. The results of the conducted experiment confirmed this hypothesis in the following table:

TABLE 10

Stimuli	Strength of the Galvanic Reflex (g.r.)			
Subjects	I K-lin	II K-lun	III K-ev	
Associated	0	7	5	First part
Associated	1	7	1	There are given color associated and differentiated stimuli. The right hand of the subject lies on the table (not touching the key).
Associated	7		1	
Associated			6	
Differentiated	1	9	68(?)	
Differentiated	5	3	2	
Differentiated	13		18	
Differentiated			3	
				Second part
Associated	7	56	205	
Associated	11	56	128	There are given color associated and differentiated stimuli. The right hand of the subject lies on the key.
Associated	5	3	40	
Differentiated	10	40	102	
Differentiated	10	3	81	
Differentiated	2	4	59	

The obtained results lead to the conclusion that one and the same stimulus has a different effect on the reacting system according to the general situation. This allows us to see in the g.r. a process determined not by the action of separate stimuli, but by the whole structure of the given situation.

In addition to the g.r. (which belong to the category of general reactions) were investigated changes in the respiratory system which are partly of a similar nature. All subjects were divided into five groups according to the type of the obtained respiratory curves, as follows:

Each group was characterized by:

I. Steady respiration,

II. Steady respiration, with reflexes,

III. Respiration which was steady at the beginning but then later became distorted,
IV. Distorted respiration,
V. Irregular, chaotic respiration.

All subjects were distributed according to the above grouping in the following way:

TABLE 11

Groups	Number of Subjects: By the Electro-dermal Method	By the 'Grabbing' Method
I	2	7
II	2	5
III	4	2
IV	8	12
V	5	4

This table makes it evident that the described types of respiratory reactions were observed in the subjects in conditions of both experiments. Only the number of the subjects who formed first or second group varied. The determined correlation between the character of respiration in conditions of both experiments showed that it was sufficiently substantial and reliable. The coefficient of the correlation $r = + 0.60$, m (error in the coefficient of the correlation) equaled ± 0.12. It was interesting to conduct a parallel study of the galvanic and respiratory reaction in conditions of the same experiment.

A number of investigators, as Jung, Peterson, Veragutt and others have studied the correlation between the galvanogram and pneumogram. These investigators believed that there is only a certain connection between the galvanic and the respiratory reflexes, but that there was no strict parallellism between them. Weinberg to the contrary stated that there exists a constant conformity between the galvanic and the respiratory reactions. The analysis of the data of our experiments was conducted on a slightly different basis. The correlation of the reactivity of these systems as a whole, was investigated, but no connection between separate reflexes of the two orders was taken into consideration. For such a comparison each subject was characterized separately for each experiment, according the peculiarities of both, the respiratory (by his belonging to one of the above mentioned five groups) and of the galvanic reactions.

The subjects were divided into five groups according to the strength of the g.r. to each stimulus: non-conditioned, associated, and differentiated; into the middle or third group were included those subjects whose g.r. equaled $M \pm \frac{1}{2}\sigma$ (sigma); into the II and IV all those whose g.r. was $M \pm$ more than 1σ (sigma). Since it was ascertained a considerable and definite correlation between the g.r. to different stimuli, it was possible to determine the belonging of each subject to one of the five adopted groups. The subjects then were definitely characterized in relation to the respiratory and g.r. according their belonging to one of these groups, as shown in following table:

TABLE 12

Subjects Belonging to the Groups	By the Electro-dermal Method; According to Character of:		By the 'Grabbing' Method; According to Character of:	
	Galvanic Reflex (g.r.)	Respiratory Reactions	Galvanic Reflex (g.r.)	Respiratory Reactions
F-nev	IV	V	III	IV
G-kin	III	I	II	I

On these data there was found the correlation between the character of the irritability of the galvanic and respiratory system in conditions of each experiment, as follows:

TABLE 13

In Conditions of Experiments by the Methods	The Correlative Signs	Coefficient of the Correlation	Error in Coefficient	Estimation of Reliability
Electro-dermal	Respiratory and galvanic reactions	+0.64	±0.10	Reliable
'Grabbing'	Respiratory and galvanic reactions	+0.62	±0.11	Reliable

Thus, was confirmed on the investigated material the existence of a definite relationship between the reactions of both sections of the vegetative system. Having examined the correlation of the galvanic and the respiratory sections of the vegetative system in different conditions, it was necessary to analyze the correlation between the vegetative and the animal reactions. Since, the grabbing method due to its peculiarities would not render sufficient data, this analysis was conducted only by the electro-dermal method, because the latter would allow a rather complete study of these correlations.

All the subjects were divided into two groups according their ability to form an associated-motor and defensive reflex. The first group, consisting of 13 subjects, showed a tendency to a rapid forming of an associated-motor reflex. The second group, consisting of 17 subjects, showed under same conditions no associated-motor irritability. Considering the peculiarities of the associated-motor nature of these two groups it appeared essential to make a comparison of their animal and vegetative reactions.

In the first group the g.r. did not show any uniformity. According to the strength of the g.r., the subjects of this group were divided in the following way:

TABLE 14

Groups	Number of Subjects	Percents
II	4	30.6
III	2	15.4
IV	5	38.6
V	2	15.4

The quoted figures show that in this group side by side with a predominance of a considerable galvanic reactivity (IV and V groups—54 percent) there were also observed to a certain extent medium and weak g.r. (III and II groups). It was important to ascertain not only the degree of irritability of the galvanic vegetative system, but also its nature; in other words, whether there was only a vegetative reaction (of a subcortical nature) or whether we were dealing with a psycho-vegetative reaction (with a corticogenic influence).

The strength of the g.r. to an associated and non-conditioned stimuli expressed in percents makes this distinction possible. The smaller the difference between the strength of the g.r. to the associated and non-conditioned stimuli is, the more does this proportion approach the mark 100, and the corticogenic influence on the vegetative reactions is indicated thereby. The size of the strength of the g.r. to the associated and to the non-conditioned stimuli was found for each subject; then the arithmetical mean of these proportions was calculated. The results showed that for the given subjects the average proportion equals $M - 47.2,6$ (sigma) $- \pm 19.29$, and $m - \pm 3.5$.

All the subjects were divided into three groups according to the index of the proportion of the strength of the g.r. to the associated and to the non-conditioned stimuli as follows: into the middle group (II) were included the subjects whose index was—$M \pm \frac{1}{2}$ 6 (sigma), *i.e.* from 56.9 to 37.6; into the group with a small index (I) were included those subjects whose index was less than 37.6; and into the extreme group (III)—those whose index was higher than 56.9.

We may presume that in the last group (with a high index) a corticogenic influence on the vegetative galvanic system was present. The group of the subjects with a rapid formation of associated-motor reflexes, was distributed according the nature of the vegetative galvanic reactions as follows:

TABLE 15

Groups	Number of Subjects	Percent
I (low index)	2	15.4
II (medium index)	3	23.0
III (high index)	8	61.6

In the last group of eight subjects (61.6 percent) the presence of a psycho-galvanic reaction could be presumed. All subjects of the second group who displayed no tendency to a rapid formation of associated-motor reflex belonged to various groups according to the strength of the galvanic reflexes.

TABLE 16

Groups	Number of Subjects	Percent
II	5	29.6
III	5	29.6
IV	3	17.4
V	4	23.4

Here the medium and the weak g.r. slightly predominate (II and III groups—59.2 percent), but a high strength of the g.r. (IV and V groups—40.8 percent) is also recorded in a considerable percentage of cases. The subjects of this group were distributed according the index of the proportion of the strength of the g.r. to an associated and a non-conditioned stimuli as follows:

TABLE 17

Groups	Number of Subjects	Percent
I (low index)	8	47.0
II (medium index)	5	29.6
III (high index)	4	23.4

In four subjects (in 23.4 percent) of this group the presence of a psycho-galvanic reaction can be supposed. *Although the associated-motor reflex is absent, the last data speak, however, in favor of formation of an associated relation.* The respiratory reflexes to an associated stimulus were met with in both groups, where an associated-motor reflex was formed, as well as in the group where the formation of such reflexes was not observed. On the other hand a respiratory reflex to an associated stimulus was frequently absent in those cases where a motor reaction was present. When studying the two indicated groups according to their respiratory reactivity, it was found that in both groups there were subjects with a varying degree of respiratory irritability.

We notice that in the motory-exited group a greater degree of respiratory irritability predominates as compared with the other group. When analyzing the obtained results, it became evident

TABLE 18

Nature of the Respiration	Formation of Associated-motor Reflexes		Non-formation of Associated-motor Reflexes	
	Number of Subjects	Subjects in Percent	Number of Subjects	Subjects in Percent
I. Steady	—	—	2	11.7
II. Steady with reflex	5	38.6	6	35.3
III. Steady becoming irregular	2	15.4	2	11.7
IV. Irregular	3	23.0	5	29.6
V. Irregular, chaotic	3	23.0	2	11.7

that in conditions of the same experiment the subjects gave reactions differing in their composition as well as in degree.

The group characterized as associated motor irritable was far from appearing as such in the sense of vegetative reactions, or it was associated-irritable only in one section of the vegetative system. On the other hand the degree of the general vegetative reaction of this group varied rather widely. The other group, which in conditions of the suggested experiment did not show any associated-motor irritability within the vegetative system, showed in a number of cases a tendency to establish an associated relation. However, there were observed extreme variations of the degree of the general vegetative reactions also in this group.

The investigated material presented some data for the following types of correlations between the animal and vegetative reactions of the different sections of the nervous system:

1. Associated-motor irritability: a weak, medium, and strong-vegetative, general or partial irritability of a simple or associated nature.

2. Associated-motor irritability absent in the given conditions: a weak, medium, and strong-vegetative, general or partial irritability of a simple or associated nature.

The grouping of all the subjects according to this scheme gives the following results.

The obtained results indicate that the subjects showed a stronger general vegetative irritability in the presence of the associated-motor irritability than in its absence.

TABLE 19

Vegetative Reactions of Galvanic and the Respiratory System	In Associated-motor Irritability		In Absence of Associated-motor Irritability	
	Number of Subjects	Subjects in Percent	Number of Subjects	Subjects in Percent
A strong vegetative reactivity of both systems	6	46.0	2	11.7
Out of these of an associated nature	4	30.8	—	—
A strong vegetative reactivity of one of systems	2	15.4	10	59.1
Out of these of an associated nature	—	—	3	17.4
A medium and weak vegetative reactivity both systems	5	38.6	5	29.2
Out of these of an associated nature	5	38.6	4	23.4

In the absence of an associated-motor irritability a strong reaction predominates in one of the vegetative system. A medium and a weak reaction of both systems is met with more infrequently in the absence of an associated-motor irritability than in its presence. *We may state that the associating nature of the vegetative reactions is observed more frequently in the presence of an associated-motor irritability than in its absence.*

According to the frequency of the encountered types of correlations between animal and vegetative reactions the subjects were distributed as follows:

1. Subjects who did not show an associated-motor irritability in the presence of strong reactions in one of the vegetative systems, only in some cases of an associated nature (10 subjects).

2. Associated-motor irritable subjects with strong general vegetative excitability, frequently of an associated nature (6 subjects).

3. Subjects associated-motor irritable and non-irritable with medium and a weak vegetative reaction, mostly being of an associated nature (5 subjects in each group).

4. Subjects associated-motor irritable with a partial vegetative reactivity, and those who did not show an associated-motor irritability with a strong general vegetative reactivity (2 subjects in each group).

Since the typical peculiarities of the reactions in this experiment were considered under conditions of an electro-dermal stimulus only, we could have no proper judgment of the type of various sections of the nervous system. The latter is understood to have a definite tendency to a rather constant character of reacting under various circumstances. The present investigation is only one of the stages of the study of the typological qualities of the nervous system.

Conclusions

1. The strength of the galvanic reflex to currents and to color stimuli varies widely in various subjects.

2. The strength of the galvanic reflex is considerably higher to currents than to color stimuli.

3. There is a considerable increase of the strength of the galvanic reflex to the associated stimulus as compared with the differentiated.

4. There is an inverse correlation between the strength of the galvanic reflex to currents and the maximal limits of the electro-dermal sensibility.

5. The same stimulus has a different effect on the reacting system (galvanic) according to the general situation.

6. The correlation between the characters of respiration in conditions of both experiments is sufficiently substantial and reliable.

7. A certain relation between the reactivity of the respiratory and galvanic sections of the vegetative system was established on the investigated material.

8. The respiratory irritability is more marked in the motory-excited group than in the other one.

9. A stronger general vegetative irritability in the subjects was observed in the presence of an associated-motor excitability than in its absence.

10. The associating nature of the vegetative reactions is observed more frequently in the presence of an associated-motor irritability than in its absence.

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A STUDY IN EYE-HAND COÖRDINATION

BY MEYER KORINS

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The purpose of the present investigation is to determine to what extent individuals differ in one form of motor ability; and upon what basis the differences rest. Although much work has been done on this subject, comparatively little attention has been paid to the constitutional equipment of the different individuals. Specifically stated, our problem is: will equal amounts of practice tend to produce individuals of equal ability, or will it reveal individual differences?

Investigators have offered various explanations for the improvement in motor ability with practice. It has been suggested (1) that the improvement is produced by the development of *new* mental and motor techniques—*e.g.* better adjustment, acquired knowledge, etc. (2) A second explanation emphasizes the importance of exercising the different capacities that are *already* at hand. Here then new knowledge and new techniques are insignificant as compared with the amount of practice. (3) A third theory brings in the factor of maturation. The organism grows up to a certain age and "continued exercise before the time of maturity might stimulate a more rapid growth of the capacities concerned in a function." [1] The problem of the present investigation centers upon the first two theories. No evidence, from the very nature of our experiments, is offered concerning the third theory.

In the present investigation the subjects were tested on the three-hole form board which is an instrument used to measure eye-hand coördination.[2] The three-hole form board

[1] A. I. Gates and G. A. Taylor—An experimental study of improvements resulting from practice in a motor function, *J. Educ. Psychol.*, 1926, **17**, p. 227.

[2] A similar apparatus is referred to in C. H. Stoelting Co., 'Psychological and Physiological Apparatus and Supplies,' p. 59, Instrument No. 19223.

is a triangular board having holes in the corners 5 mm in diameter and 15 cm apart. A stylus, which is to be held like a lead pencil, is to be inserted as rapidly as possible into the holes in clock-wise order. Contacts are made when the stylus touches the bottoms of the holes and they are recorded on an electric counter. The score of every subject for each series was, in the present experiment, the number of contacts made per minute.

Subjects

Three groups of subjects were used. The first group (*A*) consisted of 22 athletes representing Columbia College in basketball and fencing, these sports involving the use of eye-hand coördination. The second group (*B*) consisted of 25 athletes selected from among the runners, high-jumpers and swimmers—sports that do not involve the coördination of eye and hand.[3] The third group (*C*) consisted of 36 men who were non-athletic. No member of this group had ever engaged in a sport involving eye-hand coördination. Furthermore every individual was questioned regarding his skill and the extent of his participation in certain sports not listed in the activities of the college, the playing of certain musical instruments, motor activities in industry, etc. In this manner, a fairly good estimate of every man was obtained in regard to previous training in eye-hand coördination. To be sure, it would be impossible to find an individual lacking *some* experience in eye-hand coördination. An attempt was made, however, to choose only those individuals for groups *B* and *C* whose eye-hand coördination was no greater than average. Doubtful cases were not accepted.

The total number of subjects used in the three groups was 83, the range of ages being from 17 to 24 years. All testing was done during the late afternoons.

Procedure

A uniform method of procedure was used throughout. The investigator showed each subject what was expected of

[3] It might appear that such coördination exists in swimming but an analysis reveals the fact that the eye serves the same function here as in walking. One can easily swim with eyes closed, yet playing basketball or fencing would be impossible in this manner.

him as regards the inserting of the stylus. No practice was permitted. All were told to work as accurately as possible, beginning to work at the signal 'go' and stopping at the signal 'stop.' Five one-minute series were given, with an interval of one minute rest between the series.

The average scores and sigmas of the three groups are as follows:

Group	No. Cases	Av. Score	Sigma (dis.)
A	22	127.1	5.57
B	25	121.6	9.79
C	36	113.3	11.27

The reliabilities of the differences between the different groups were as follows:

	A and *B*	*B* and *C*	*A* and *C*
Diff./sigma diff.....	2.4	3.1	6.2

True differences exist between the athletes and non-athletes and probably also between the two groups of athletes. A diff./sigma diff. = 2.4 means that the chances are 99.2 in 100 that the superior group will remain superior when new samples are taken. It would therefore be permissible to assume a true difference between these two groups of athletes. The probabilities of true differences are even higher in the other cases.

How are we to account for the differences between the three groups? The reliability difference between groups *A* and *C* is 6.2, which is more than twice that which is necessary to insure complete reliability. Since group *A* was comprised of athletes having training in the coördination tested, it might be argued that this experience accounts for the group differences. From Fig. 1 we see that groups *A* and *C* reach their approximate limits at the end of the fifth trial. It is difficult to say whether or not group *C* might have gained, after this apparent limit, through new methods, etc., alluded to before,

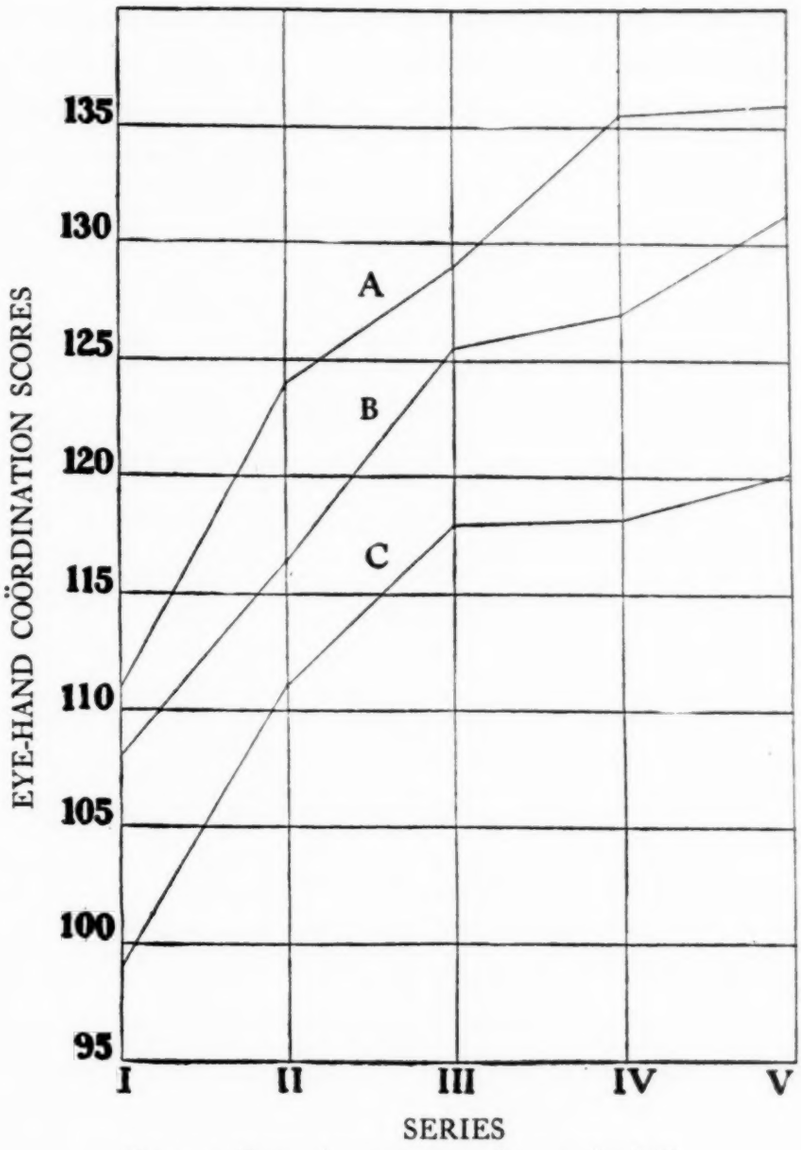

FIG. 1. Group improvements from series I–V.

and eventually reached the ultimate limits to be attained by group *A* after both groups had been given an infinite series. This extended series would enable group *C* to have the benefit of an unlimited amount of practice.

Looking at it from another viewpoint, we may consider the results of group *B*, which is really a control group. If the method used, in determining the extent of previous training in eye-hand coördination, has enabled us to have two equal groups (*B* and *C*) in this respect, then the differences between the two groups may be due to something innate.

The differences between the first and last scores made by the three groups are as follows:

Group	No. Cases	Effects of Practice (Series I–V)	
		Gains	Percentage Gains
A	22	25.2	22.7
B	25	23.4	21.7
C	36	21.1	21.3

The reliabilities of the differences, in the average scores of the groups, between the first and last series are as follows:

	A and *B*	*B* and *C*	*A* and *C*
Diff./sigma diff.....	.48	.66	1.07

The differences between the three groups are actually unchanged by practice; instead of making individuals equal in a given function, equal practice does not affect group differences.

The question might be raised whether or not the members of groups *B* and *C* could become fencers and basketball players through sufficient practice. It is very doubtful whether they would ever attain the efficiency of group *A*. Technique, new knowledge, adjustments, greater facility, etc., might take them a certain way; then a limit would be reached beyond which the constitution of their motor and neural mechanisms would not permit them to go. At the same time, it is readily admitted that a good deal of training must have been necessary to make the men in group *A* efficient athletes. Their constitutions were able to benefit from such training, knowledge, etc.

A. I. Gates [4] remarks: "The degree of absolute achievement at any stage may be increased by special practice or experience but only within certain limits which are determined by native endowment. When practice and other circumstances of life are equalized, differences between individuals

[4] A. I. Gates—*Elementary Psychology*, pp. 111–112.

will still exist and these differences are native." This theory of equalized training and unequal achievement can be directly applied in this analysis of our results.

TABLE I

AVERAGE GROUP SCORES FOR SERIES I–V

	No. Cases	Av. Score Series 1	Av. Score Series 2	Av. Score Series 3	Av. Score Series 4	Av. Score Series 5
Group *A*.....	22	110.8	124.0	129.0	135.5	136.0
Group *B*.....	25	107.9	116.4	125.5	127.0	131.3
Group *C*.....	36	99.0	111.2	117.9	118.2	120.1

Group differences were uniformly maintained throughout this experiment. Varsity basketball players and fencers (Group *A*), varsity swimmers, high jumpers and runners (Group *B*), non-athletes (Group *C*) were just as dissimilar in eye-hand coördination scores in the fifth as in the first of five one-minute work periods. Percentage gains from the first to the second, from the first to the third, from the first to the fourth, from the first to the fifth periods are approximately equal for the different groups as shown in Table II.

TABLE II

	Percentage Gains				
	Series I and II	Series I and III	Series I and IV	Series I and V	Series IV and V
Group *A*	11.9	16.4	22.3	22.7	0.4
Group *B*	7.9	16.3	17.7	21.7	3.4
Group *C*	12.3	19.1	19.4	21.3	1.6

In view of the shortness of the experiment, owing to limitation of time available to the athletes who had participated in this experiment, it would be difficult to conclude as to a comparison of the groups with further extension of the series. This is important for a valid answer to the question already raised as to the factors involved in eye-hand coördination skill. We may, in view of the statement of Gates just cited, suggest that since the practice in this experiment had been equalized, superiority in constitutional equipment may explain to a certain extent the consistent superiority of the

varsity basketball players and fencers (Group *A*) at least over the non-athletes (Group *C*).

Constitutional or training factors may explain the differences found between Groups *A* and *B*, and between Groups *B* and *C*. The shortness of the experiment makes it difficult to definitely explain the fact that throughout the experiment Group *B* was superior to Group *C* but inferior to Group *A*.

Summary

(1) In an experiment on eye-hand coördination, a group of athletes, with previous practice in the function studied, made better scores than another group of athletes, without previous practice, who, in turn, made better scores than a group of non-athletes without previous practice.

(2) Eye-hand coördination scores were uniformly higher in five one-minute periods for varsity basketball players and fencers (Group *A*), than for varsity swimmers, runners and high jumpers (Group *B*). In turn, scores of Group *B* were uniformly higher for Group *B* than for the non-athletes (Group *C*). Equal percentage gains are made by all groups from one series to another.

(3) Besides such factors as new knowledge, new technique, better adjustment, etc., the constitutional nature of the motor and neural mechanisms will determine improvement in eye-hand coördination.

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APPARATUS FOR THE STUDY OF CONTINUOUS REACTION

BY P. E. HUSTON AND J. G. HAYES*

A number of techniques have been developed for the purpose of studying the continuous reaction of a subject to a variety of stimulus situations. These methods have been reviewed recently by Seashore (3). Obviously such techniques may be applied in a number of instances, being particularly suited for studying fluctuations in a subject's condition (1). The two arrangements described herein are somewhat similar to an apparatus used by Miles (2) to measure neuro-muscular efficiency in connection with problems of nutrition and alcohol ingestion. Since our apparatus are less expensive and the parts more easily obtainable than that of Miles we offer a description of them.

Figure *I-A* gives a schematic diagram of one arrangement. The essential part of the apparatus consists of a Wheatstone bridge. One arm (R_m) of the bridge is continuously unbalanced by a motor-driven slider. The pattern traversed by the slider is determined by a system of three worm gears of 20, 40, and 96 teeth driven from the same shaft. Small metal rods are fastened on the sides of these gears near their periphery. A strong, flexible cord attached to one end of the slider on R_m passes over two of these metal rods and is tied with a loop to the third [see Miles (2)]. This line is kept taut by a weight attached with a cord over pulleys to the other end of the slider. Thus, as the gears revolve, the slider is drawn back and forth in a pattern set by the rotational speeds of the gears and the distances of the metal rods from the gear centers. This pattern is such that the slider changes its speed, its extent of movement, and its direction frequently. With every 480 revolutions of the drive shaft the pattern repeats itself.

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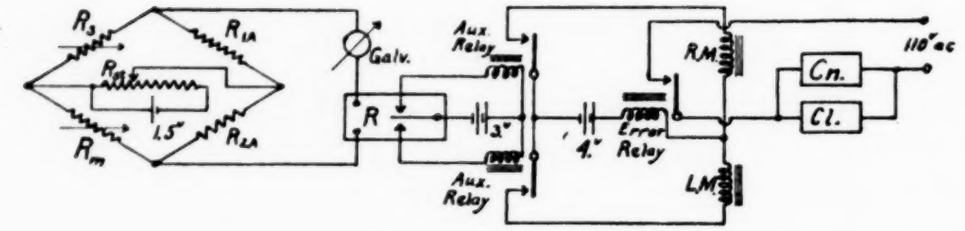
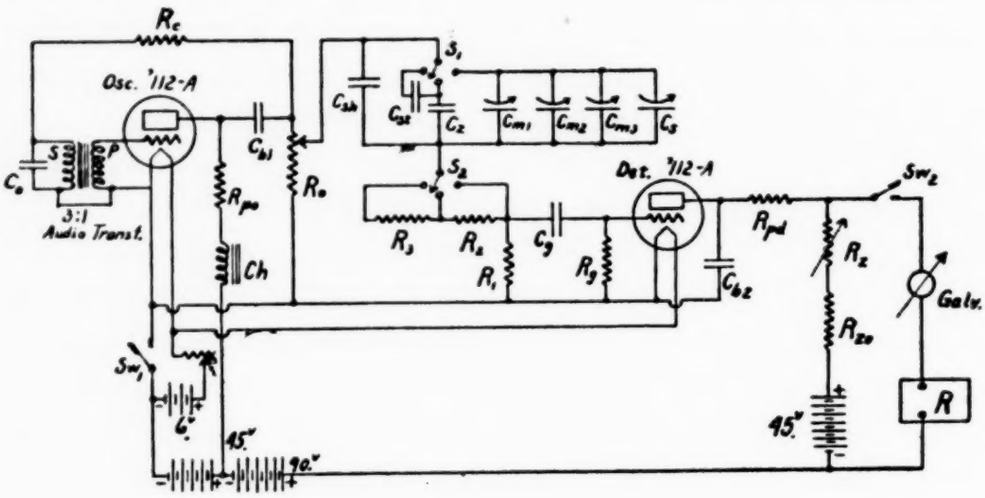


I-A



I-B

TABLE I

I-A		I-B	
R_s	= 0 to 500 Ohms	R_c	= 1.0 Megohm
R_m	= 0 to 500 Ohms	R_{po}	= 5,000 Ohms
R_{1A}	= 500 Ohms	R_o	= 200,000 Ohms
R_{2A}	= 500 Ohms	R_1	= 200,000 Ohms
R_{st}	= 1400 Ohms	R_2	= 200,000 Ohms
		R_3	= 200,000 Ohms
Recording		R_g	= 8.0 Megohms
		R_{pd}	= 5,000 Ohms
Galv.	= 660-0-660 microampere galvanometer, Weston Model 375.	R_z	= 0–10,000 Ohms
		R_{zo}	= 10,000 Ohms
R	= Polarized relay, Weston Model 30, 1100 ohms.	Ch	= 30 Henry, 350 Ohm Choke
		C_o	= .0005 mfd.
R.M.	= Right error kymograph marker winding.	C_{b1}	= 0.5 mfd.
		C_{sh}	= .0002 mfd.
L.M.	= Left error kymograph marker winding.	C_{st}	= .002 mfd.
		C_z	= .0015 mfd.
C_n	= 110 Volt A.C. counter	C_{m1}	= 18 Plate S.L.C. condenser
C_l	= Telechron clock.	C_{m2}	= 13 Plate S.L.C. condenser
		C_{m3}	= 14 Plate S.L.C. condenser
		C_s	= .0013 mfd. (.001 mfd., S.L.C. and .003 mfd. S.L.F. in parallel)
		C_g	= .01 mfd.
		C_{b2}	= .25 mfd.

The automatic unbalancing process deflects the needle of a zero-center scale galvanometer to the right or left, the subject's task being to keep the needle at the zero position by means of the slider on R_s which maintains the bridge in balance. In order to keep the needle in a central position it is necessary for the subject to move his slider with such a speed, extent, and direction as will match the movements of the automatically driven slider, R_m. The level of task-difficulty is determined by the speed of the shaft which drives the gears. At the present time this is set at 25, 37.5, and 50 R.P.M. The pattern thus repeats itself in 19.2, 12.8, and 9.6 minutes, respectively. Due to the complexity of the pattern, there is no opportunity for the subject to anticipate the movement of the needle except when his slider is at either extremity of its excursion.

The total possible movement required from the subject is 4¾ inches, R_s being 5⅝ inches long (R_m was constructed indentical to R_s). The subject's slider is made as frictionless as possible; only a light touch is necessary to move it. R_s is mounted on the under side of a table top with the slider knob projecting through a rectangular slot. R_{st} varies the voltage applied to the bridge; hence a standard deflection can be obtained from day to day as well as changes in the level of difficulty.

The recording is accomplished through the polarized relay, R, which is adjusted so that when the subject permits the tip of the galvanometer needle to deflect by more than one millimeter (this allowance can be varied to meet the requirements of the experiment) to either side of zero, the auxiliary relays operate. The auxiliary relays are wired to a two-way kymograph marker, one relay indicating needle deflections to the right of zero, the other, deflections to the left. In the common lead of the marker is a relay which operates an electrical counter and a Telechron clock. Thus a graphic record of the number of errors (right and left deviations by one mm., or more from center) and the time per error is made, as well as a cumulative score of the number of errors and error times.

The arrangement *I-B* contains the automatically varied capacities C_{m1}, C_{m2}, and C_{m3}. These are rotary air condensers of different capacities driven by the same worm gears as in method *I-A*. C_{m1} is rotated by the 96-tooth gear, C_{m2} by the 40-tooth gear, and C_{m3} by the 20-tooth gear. These condensers, in series with a fixed resistance R_1, are across the output of an audio-frequency oscillator. As these condensers rotate, their impedance is varied, so that the voltage drop across R_1 changes. This varying voltage is placed on the grid of the detector tube, thus altering its plate current which operates the same galvanometer and polarized relay as in set-up *I-A*. The recording apparatus is also the same as in *I-A*. In order to keep the galvanometer needle on zero the subject must adjust the variable capacity, C_s to keep the sum of capacities C_{m1}, C_{m2}, C_{m3}, and C_s constant, at a value about equal to one-half the sum of the individual maximum capacities.

The audio-oscillator (frequency about 800 cycles) is used instead of the ordinary 60 cycle current since the inertia of the armature of the polarized delay *R* is insufficient to prevent chattering. It is also necessary to separate the leads of the variable condensers, C_s, C_m, to reduce the shunting effect of the distributed capacity of the leads.

The variable resistance R_z and the fixed resistance R_{z0} in series with the 45-volt battery across the output gives a current in the galvanometer circuit for the purpose of balancing the D.C. plate current in the galvanometer circuit when S_1 is on its central position so that the galvanometer needle is in a zero position. S_1 in its central position substitutes a fixed capacity C_z, which equals approximately one-half the sum of the capacities, C_{m1}, C_{m2}, C_{m3}, and C_s. This procedure causes the plate current in the galvanometer circuit to vary about zero current, or the galvanometer needle about its central position when S_1 is on the right tap which is its position during operation. C_z was determined empirically. With S_1 on the right position, R_z was adjusted with C_{m1}, C_{m2}, C_{m3} in maximal and minimal pattern deflection positions until it was necessary to move C_s equal amounts to the right

or to the left of its central position to bring the galvanometer needle to zero. S_1 was then set on its middle tap and without altering the value of R_z, C_z was set to give zero galvanometer current.

When S_1 is in its right-hand position and with C_{m1}, C_{m2}, and C_{m3} in their maximal pattern-deflection position and C_s in its central position the galvanometer deflection is 198 micro-amperes or 9 millimeters. With C_{m1}, C_{m2}, and C_{m3} in their minimal pattern-deflection position and C_s in its central position, the deflection is 429 micro-amperes or 19½ millimeters in the opposite direction. S_2 is set on its right-hand tap for these values.

In order to adjust R_0 to give the above standard conditions, regardless of variations of circuit conditions, S_1 is moved from the central to the left-hand position and the needle deflection from zero is made uniform by adjusting R_0 to give the same deflection at each calibration. The value of this deflection will be determined by the capacity of C_{st}.

The purpose of S_2 is to adjust the sensitivity of the output apparatus by applying fixed proportions of the voltage changes to the grid of the detector tube. This procedure decreases or increases the extent and speed of the galvanometer needle movement, thus providing three levels of difficulty without changing the form of the pattern. (Changing the speed of the motor which drives C_{m1}, C_{m2}, and C_{m3} varies solely the speed of the needle movement.)

In order to rotate the subject's condenser through 180° and at the same time to render easy the movement required of the subject, his condenser is mounted on the under side of a board 20 inches long and 17 inches wide and the condenser is rotated by a rigid metal rod pivoted on the same side of the board. When this rod is moved through an angle of 50°, C_s is revolved 180° by spur gears. This rod has its end bent at right angles and projects through a slot ⅜ inches wide in the board. A small wooden knob for the subject (free to revolve on the metal rod) is mounted on this projection of the metal rod. Thus the total possible movement of the subject's knob is about 8¾ inches on the arc of a circle, the

radius of which is 10 inches. The board which holds the condenser C_s is tilted at an angle of about 25° from the table and the subject's galvanometer is placed 3 inches from the knob when the latter is in a central position. Thus the subject can comfortably rest his forearms on this board while moving the knob and easily view the galvanometer needle. The scale which is supplied with the galvanometer by the manufacturer was removed and a paper of medium grey substituted. The grey paper helps reduce the visual contrast effects. A small black triangular shaped mark on the paper indicates the zero position. Daylight illumination is provided for the subject. The recording apparatus and the automatic pattern-making devices are placed in another room from the subject to eliminate noise.

Arrangement *I-B* possesses some advantages over *I-A*, viz., that since there are no sliding contacts in *I-B* no trouble can be experienced from this source. A somewhat smoother pattern variation is also obtained from *I-B*.

In one important respect these methods differ from that of Miles. Because his apparatus includes integrating watt-meters for recording, the subject's score depends not only upon a deviation from center but also upon the amount of this deviation. The further the subject is from maintaining the needle on a central position the faster the watt-meters record. Our time score may reflect this inadequacy of pursuit to some degree since the further the subject is from center the longer it will take him to get back—hence the longer will the Telechron clock operate. In our records there is some opportunity to distinguish subjects who pursue too slowly from those who accumulate errors due to momentary attention lapses since a ratio of Time over Errors would be larger in the former case than in the latter. However, if the subject combines both types of performance, or one in which he moves his slider or knob too far so as to over-compensate, the error score is increased and it becomes more difficult to distinguish between subject types. The Miles apparatus, if his graphic recorder is not added, makes no distinction between errors and time, although it might be possible to

add an error counter to it. We have felt it advisable to keep the number of errors and the time of the errors separate, since upon retest much greater consistency has been found in the time score than in the error score. Preliminary work on twenty schizophrenic patients tested twice gave an intra-class correlation of 0.91 for time and 0.46 for number of errors. It is possible that the error score may be the more sensitive indicator of a person's condition (aside from learning). To attempt to evaluate minor fluctuations, we are taking at present error and error time readings every fifteen seconds for twenty minutes with two levels of difficulty.

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A DEVICE FOR DEMONSTRATING EMPATHY

BY KATE GORDON

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The concept of empathy, that is, the "feeling oneself into the object," has, for many years, played a part in theories of æsthetics. It has been held that our appreciation of the object is founded upon, or at least greatly enhanced by this seeming participation in its activity. We somehow identify ourselves with it, and so understand and value it. The idea of empathy also has relation to those theories of perception which hold that our motor response helps us to interpret the object. It is easy to cite casual instances of empathy, as the case of beating time to music. There have also been laboratory researches which show its presence. But I have often wished for some brief and simple means of illustrating the experience. The accompanying figure shows eight views of a little Mexican image which I had photographed. Four of the views were made by reversing the negatives. Observers are asked to list the eight views as

A.... B.... C.... D.... E.... F.... G.... H....

Starting with *A* they are then told to write down the answer to the question: Is it the right arm or the left arm which the statue is lifting in the air?

Some persons show marked gesticulations before they can answer the question. Others show slight movements of the hands, and some, whose movement is not publicly perceptible, report that they were aware of the twitching of their muscles.

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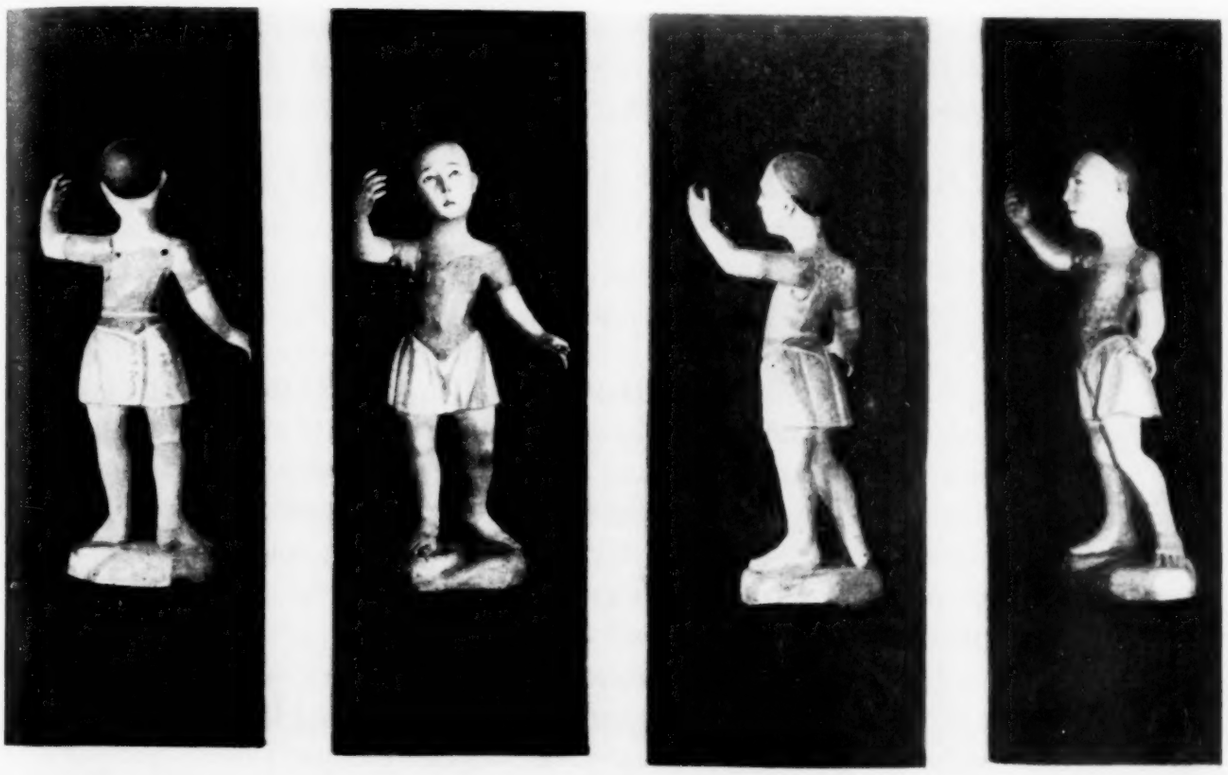
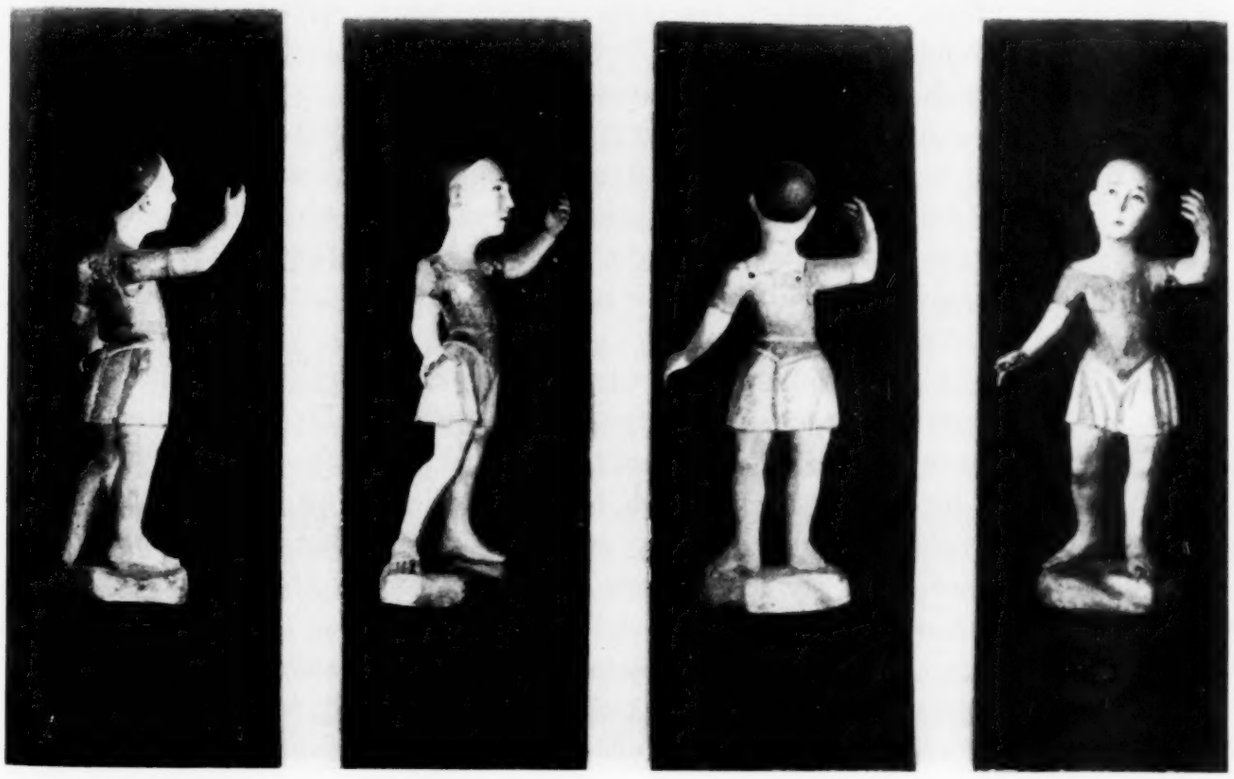
A B C D



E F G H



DISCUSSION

DR. HILGARD ON THE CONDITIONED EYELID REACTION

BY HULSEY CASON

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Dr. E. R. Hilgard has recently published the results of a study of conditioned eyelid reactions (**7**), and the description of what he did and got is a valuable contribution to the growing literature on the eyelid reaction. Dr. H's experimental report, however, is accompanied by a discussion which is not entirely convincing, and we would like to comment briefly on two general questions and one special attitude on which Dr. H may be in error.

(1) Dr. H claims that the phenomena of conditioned responses (C-R's) are more complicated than has been assumed by previous writers, and in reaching this conclusion he seems to have been principally influenced by the results of his own experiment. In reading Pavlov's book on *Conditioned Reflexes* (**8**), some have gained the impression that the subject is unnecessarily complicated especially on the theoretical side, and that any kind of legitimate simplification would be a scientific advantage. The factors involved in Dr. H's experiment were not complicated compared with those described by the Pavlov school, and those who have worked on reflexes in the Sherrington tradition have been aware that their subject was complicated and difficult.

Studies have frequently been made of eyelid reactions in which a full closure of the eyelid occurred, and although Dr. H includes some of these complete eyelid reactions in his study, he seems to be especially interested in twitches of the eyelid. The smaller the twitches the more they seem to appeal to him. But the organic principles which apply to twitches may not apply in the same way to complete eyelid reactions.

Whether one studies the general frequency of natural winking; whole eyelid reactions; reactions of the upper eyelid, lower eyelid, or third eyelid (See **2**); abortive winks or twitches; the organic functions of the various sensory and motor nerves involved in winking; or action current studies of some of the individual nerve fibers

involved in winking; etc. would depend upon the theoretical and scientific problems being investigated. The frequency of natural winking might be a useful object of study in connection with one problem, it might be sensible to use complete eyelid reactions in another problem, and an action current study of some nervous or muscular functions might have a bearing on a third theoretical issue. One would probably not study reactions of the third eyelid just to be studying them, because the principal emphasis is generally placed on the theoretical and scientific question being investigated. Schlosberg carried out a fine study of C-R's in which one knee-jerk was employed (**10**), and Wendt carried out a fine study of C-R's in which both knee-jerks were employed (**11**). Now is there any reason why all investigations should be limited to partial knee-jerks? Dr. H appears to assume that in investigating conditioned eyelid reactions, there is a special value in studying small twitches of the eyelid, but it is not immediately obvious why it is better to use a partial eyelid reaction than a complete eyelid reaction, or why it is better to use a reaction of one eyelid than the reactions of both eyelids. In this study of eyelid reactions in animals, Blount (**2**, 111–112) has distinguished between the following types of eyelid reactions: blink, wink, half-blink, pseudo-blink, pseudo-wink, flicker, flink, and full-closure.

(2) The claim that C-R's and voluntary reactions are different kinds of responses involves the belief that there is a difference or that there are several differences between the two kinds of responses. One might use both verbal expressions and believe at the same time that C-R's are voluntary responses, or that voluntary responses are C-R's, or that neither are what many consider them to be. Dr. H assumes that C-R's and voluntary responses are two different kinds of responses, and he discusses the question of whether certain of the reactions which he obtained were C-R's or voluntary responses (Cf. **9**). He describes several differences between voluntary responses and what he considers to have been C-R's and then asserts that no essential differences between the two kinds of responses were observed. Wendt also stated that C-R's and voluntary responses were different and then said they were the same: "Although voluntary reactions were found to have greater range than conditioned responses, no essential differences between the two kinds of responses were revealed" (**11**, 82).

The speed of C-R's may be faster or slower than the speed of voluntary responses, but it would be surprising if the speeds of the

two kinds of responses were exactly the same. The amplitude of C-R's may be greater or less than the amplitude of voluntary reactions, but it would be accidental if the amplitudes of the two kinds of responses were exactly the same. It would also appear difficult for a single experiment to prove that there never are any differences between the two kinds of responses.

(3) Dr. H does not emphasize the differences between his results and some of those previously described by Dodge on C-R's (**6**), reflexes, inhibition, and learning; but wherever his results and those I obtained in 1922 are not the same, Dr. H concludes without hesitation that I was wrong. In discussing his results and mine, Dr. H seems to neglect a number of important differences in procedure to which I would like to call attention.

(*a*) Dr. H (**7**) was principally interested in twitches of the eyelid, and I was studying (**3**; **4**) practically complete eyelid reactions. In one of the figures stressed by Dr. H and regarded as somewhat representative (**7**, 13), there were as far as can be judged only 2 complete eyelid reactions and 88 partial eyelid reactions. Is this the most common type of eyelid behavior?

(*b*) Dr. H called out original eyelid twitches by a loud sound, and attempted to condition these twitches to a 30 to 50 meter candle increase in the illumination of a visual field. I called out practically complete eyelid reactions by an electric shock, and attempted to condition these winks to a sound. Dr. H's conditioned light stimulus called out reflex twitches in all of his S's 'more or less frequently' (p. 26) before his C-R training began. My conditioned click sound stimulus did not call out reflex winks in any S's before the C-R training began. Dr. H said that his (mouse-trap) sound apparatus was an effective unconditioned stimulus for a reflex wink, but that the eyelid reaction was small for some S's (p. 4). On page 10 he said the 'unconditioned reflex' was regularly present from the start. I have tried to call out reflex winks by a loud sound, and have found that a loud sound does not repeatedly call out reflex winks. On page 3 Dr. H said his pendulum-photochronograph apparatus was noiseless, but on page 8 he said it made a sound. The shutter which produced the light stimulus seems to have made a sound. Cohen (**5**) and Peak (**9**) have described some improvements in the apparatus.

(*c*) Dr. H used relatively few repetitions of the C-R training stimuli, the exact number being difficult to determine from his report; and I used a large number.

(*d*) Dr. H had his S's return for short C-R training periods on different days, and I used one continuous training period.

(*e*) Dr. H seems to have carried out an experiment on backward conditioning (p. 14), but he does not give his results. On page 27 he also refers to 'Figure 7,' which is not included in his report.

(*f*) Dr. H does not give the complete results of any one of his S's and I gave the complete results of 5 S's.

(*g*) Dr. H's conditioned light stimulus could not be given while the eyelids were shut, and my conditioned sound stimulus could be given at any time. His conditioned light stimulus was influenced by what his S's were and had been looking at.

(*h*) Dr. H's S's were in a strained, awkward, and unnatural position (p. 4). My S's were comfortable and could move their eyes and head, shift their bodily position, and breathe freely.

(*i*) Before each repetition of the C-R training stimuli, Dr. H's S's were put through a ritual of 'inhale-exhale-inhale-hold,' etc.; and they gazed at the visual stimulus. My S's were given no ready signals of any kind.

Is it not possible that the differences in conditions mentioned above may account for some of the differences in results?

Some of the complications in Dr. H's experiment seem to have been the result of using the cumbersome photographic method, using sound as the conditioning stimulus, and using light as the conditioned stimulus. Since the eyelid moved in a curved line and the slit admitting light to the film was straight, the record obtained on the film could not accurately represent the distance moved by the eyelid, and some of Dr. H's statistical treatments of eyelid amplitude are misleading.

Dr. H's experiment on the influence of giving the light stimulus simultaneously with or at different times before the sound (in the C-R training) was carried out on one subject (D) who came for 21 sessions on as many different days. This experiment is described in Chapter V, which carries the title 'The Minimum Latency of Conditioned Eyelid Reactions to Light.' It is doubtful whether an experiment on this topic with one S has any clear meaning because of the effects of previous experience and training, verbal habits, sets, etc. The results are quite irregular and some of the differences are unreliable. This experiment does not show that giving the conditioned stimulus simultaneously with or .050 sec before the conditioning stimulus is unfavorable for conditioning, or that the physiological law of contiguity is wrong.

Finally, we are left with a feeling of uncertainty as to whether all of the eyelid reactions which occurred in Dr. H's experiment were not reflexes or voluntary reactions. On page 11 he says that only 5 of his 8 S's were conditioned; two S's had very small unconditioned reflexes to sound, and one S gave unusually large reflexes to light at the beginning of the experiment. On page 26, however, he says all of his 8 S's gave a reflex response to light 'more or less frequently' at the beginning of the experiment. Light was the stimulus to which the eyelid reactions were supposed to be conditioned.

The practical identity between what was thought to have been one of the average C-R times of subject D (p. 19) and the average voluntary reaction times of 4 S's (pp. 24–25), D being included among these 4 S's, suggests that all of these eyelid reactions may have been voluntary reactions. Compared with the results obtained by Allison (1, 61–65) and the writer (3, 4) on voluntary winking time, the figure of 167σ obtained by Dr. H appears small even when differences in procedures are considered. Peak's finding that the average closing velocity of voluntary eyelid responses is greater than that of reflexes (9, 152–153) makes this figure even more difficult to understand. Can unpractised S's wink within 167σ without a ready signal?

When conditions were arranged for the purpose of studying voluntary reactions, it was found that, compared with what Dr. H had regarded as C-R's, the voluntary reactions had a greater amplitude and a longer and more variable reaction time. This seems to suggest that when Dr. H thought he was obtaining true C-R's, his S's were working under a loosely controlled verbal set to react voluntarily. He seems to reach some such conclusion as this himself on pages 34, 35, and 48. He does not give any photographic records of the voluntary eyelid reactions which he obtained, but with the results of Peak's study (9) of reflex and voluntary reactions of the eyelid now available, it should be possible for Dr. H to reëxamine his photographic records and decide more definitely whether what he thought were C-R's in his experiment were voluntary reactions or not.

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THE LATENCY OF CONDITIONED EYELID REACTIONS: A REPLY TO DR. CASON

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According to Dr. Cason's findings, the average latency of conditioned eyelid reactions to sound is intermediate between the latency of reflexes to sound and voluntary responses (**2**) (**3**). In my experiment, the mean latency of conditioned eyelid reactions to light was found to lie either (*a*) at reflex latency, or (*b*) at a latency comparable to voluntary reactions to light. Responses of the latter latency were much the more common type of conditioned reaction (**9**). My failure to find responses of intermediate mean latency is the sole point of significant disagreement between Dr. Cason's results and mine, and discussion to be relevant must be directed to this issue. I regret that when Dr. Cason chose to criticize my study he signally overlooked the opportunity to comment upon my only specific criticism of his experiment. At the beginning of my monograph (**9**, 2) I pointed out that the latency distributions reported by him for conditioned and voluntary responses gave internal evidence that the two series were not comparable, and that the results of his study were therefore indeterminate. This analysis did not set my findings against his, and could have been made quite as cogently if my experiment had never been performed. I feel called upon, therefore, both to defend myself, and to reiterate my analysis of his data.

Reply

I shall reply to four of Dr. Cason's propositions, which I restate as I understand them: (1) that because of my interest in submaximal responses I may have missed important organic principles which apply only to *complete* eyelid reactions; (2) that differences in our procedures may have been the cause of the *significant* differences in our results; (3) that the results of my experiment on one subject in the section entitled 'The Minimum Latency of Conditioned Eyelid Reactions to Light' are inconclusive, and the deductions misleading; (4) that I have been confused both in experimental analysis and in my thinking regarding the relationships between conditioned and voluntary responses.

(1) *Submaximal Responses ('Twitches').*—Representative of the type of analysis made possible through the use of graduated eyelid closures is the curve of acquisition of conditioned responses which I presented as *A* in Fig. 3, page 14 of my monograph (9). This shows the increase in daily amplitude through a protracted conditioning series. Study of such significant relationships requires submaximal responses. Dr. Cason's penchant for complete closures is illustrated in his inaccurate statement that eyelid reactions are generally 'all or none' (2, 153), and in his assertion in the course of his criticism that it is difficult to elicit reflexes to sound with regularity, in spite of the abundant experimental evidence of Dodge (5) (6) (7), Miles (15), Peak (16) (17), Cohen (4). The issue, however, is whether or not my results are due to the particular stimuli which I employed. Bernstein (1), in a recent study of conditioned eyelid reactions [1] used sound as the conditioned stimulus, shock as the unconditioned stimulus, both similar to Dr. Cason's stimuli, and his results, by his own testimony (1, 182, 189) confirmed mine. With the collaboration of Mr. Albert A. Campbell I have recently secured thousands of records in which an air-puff to the cornea served as the unconditioned stimulus. This should satisfy Dr. Cason, because 'complete' closures were obtained consistently from many of our 63 subjects. The study will appear in time; for the present permit me to state dogmatically that no important correction of my previous discussion will be found necessary on the basis of these records.

(2) *Differences in Procedure.*—Experiments as diverse as Hamel (8) and Humphrey (14) on finger retraction, Schlosberg (20) and

[1] Bernstein's experiment was carried out in the Oberlin laboratory under the supervision of Dr. L. E. Cole, and neither Dr. Cason nor I had anything to do with it.

Wendt (**21**) on knee-jerks, Hudgins (**13**) on pupillary responses, Bernstein (**1**) on eyelid reactions, are all in agreement with me that the latency of conditioned reactions is typically indistinguishable from that of voluntary responses. Dr. Cason cannot argue against these findings by merely pointing out differences between his experiment and mine. He has patently failed to show how the differences account for the specific latency disagreement. Bernstein used Dr. Cason's stimuli and *both* our time intervals, so his experiment nicely arbitrates our differences. Dr. Cason has criticized my subjects' position and my breathing instructions; Bernstein used a recording device which permitted all the freedom of Dr. Cason's apparatus, he used no breathing instructions or other ready signal, and his results were like mine. Therefore these particular criticisms of my experiment prove to be irrelevant. For those who wish to check further on Dr. Cason's comments regarding my experimental procedure, I refer to the following pages in the monograph (**9**): pages 3 and 8, for the fact that I did not contradict myself in ascribing noiselessness to the pendulum; Fig. 7 is an insert opposite page 27; breathing instructions were defended on page 7; the distortion of amplitude readings is pointed out on page 4. Latency readings, with which we are most concerned, are free from any distortion, other than the ever-present one of determining the *exact* point at which a record line departs from its baseline. The differences between our experiments are obvious; Dr. Cason should have shown specifically how such differences accounted for disagreement in our conclusions.

(3) *Minimum Latency of Conditioned Responses; the Law of Contiguity.*—Instead of arguing the logic of including the results from my experiment on one subject,[2] let me appeal to the facts revealed in Bernstein's more extensive study of the same problem. He found through experimenting with many subjects at a number of intervals favorable for the formation of conditioned responses at low latency, that the minimum latency of conditioned responses agreed with the minimum latency of voluntary responses (**1**, 180). This was my contention. He shows also (what Dr. Cason believes I have not shown) that giving the conditioned stimulus simultaneously with, or .050 sec before the unconditioned stimulus, *is unfavorable* for conditioning eyelid responses. Wolfle has demonstrated facts of the same order for conditioned finger withdrawal (**22**).

[2] I had confirmatory evidence from two other subjects as reported in footnote 13, page 10 (**9**).

These are the experimentally revealed facts: one can infer what one wishes with regard to the physiological law of contiguity. Contiguity may mean direct succession, a relationship possibly fulfilled by the relatively short favorable intervals of Wolfle and Bernstein; if contiguity is to remain the most favorable interval for conditioning, it can no longer be taken to mean physical simultaneity of the stimuli. I have elsewhere shown that simultaneity is an ambiguous concept, unless one is told whether it refers to stimuli, to responses, or to something else (**10**, footnote 4, 97; 107).

(4) *Conditioned Responses and Voluntary Responses.*—Dr. Cason remarks that Wendt and I believe that conditioned responses may be both *different from* and *the same as* voluntary responses. He later refers to Dr. Peak's able experimental study of reflex and voluntary reactions (**18**); I should like to refer also to her theoretical treatment (**19**). She points out that responses may be alike in their *descriptive features* (*e.g.*, latency), and unlike in their *functional relations*. Conditioned and voluntary responses may therefore be both alike and different. If by *some* criteria they be found indistinguishable, does it become necessary for me (along with Hamel, Humphrey, Schlosberg, Wendt, Hudgins, and Bernstein) to admit that we were not dealing with conditioned responses?

That there need be no confusion between the reflexes and the conditioned responses to light as obtained in my experiment, I refer to a figure showing the appearance of the two responses on a single photographic record (**11**, 210), and to a latency distribution of 748 reflexes and 688 conditioned reactions to light (**9**, 26). If Dr. Cason implies that my conditioned responses were all voluntary because I found no third type of response with a latency between that of reflexes and voluntary reactions, attention may be called again to the other studies which have failed to find this intermediary (**1**) (**8**) (**13**) (**14**) (**20**) (**21**).

Dr. Cason questions the reasonableness of my value of 167 σ as a mean of voluntary reactions to light, believing the value to be too low for responses without a ready signal. He fails to note that my breathing instructions served as a ready signal, as I pointed out explicitly (**9**, 24). When Dr. Cason's own latency figures for voluntary reactions to sound *without a ready signal* are corrected pursuant to his suggestion (**3**), it is found that 67 of 150 reactions, or 45 percent, occur at latencies below 170 σ for the five subjects for whom all data are given, and the mode lies between 140 and 160 σ. (See Table I.) Although reactions to light usually show longer latencies

than reactions to sound, Dr. Cason's own data make my figure of 167 σ reasonable for reactions to light under my instructions.

Dr. Cason inaccurately states that in my study voluntary reactions had a 'longer and more variable' reaction time than conditioned responses. The differences were, in fact, statistically insignificant; at least for variability the difference is in the other direction (**9**, 25). In fairness to Dr. Cason, it appears that this statement may be a slip of his typewriter, for his argument seems to require the facts as I reported them, not as he stated them. I agree with his argument that my subjects probably developed a loosely controlled 'set' to react; I am not as sure as he that it was a *verbal* set, for Dr. Marquis and I have found that in a comparable conditioning situation *dogs* behave very similarly (**12**).

Critique

In order to be perfectly clear in repeating the criticism which I offered of Dr. Cason's study, a criticism which he has left unanswered, a series of frequency distributions from Dr. Cason's raw data are presented in Table I.

The latency correction has been introduced on the basis of Dr. Cason's note (**3**). The latencies of all reflexes to the sound-shock combination for the five subjects whose responses are reported in Table I yielded uncorrected mean values of 95, 103, 121, 127, and 131 σ. These were obviously too long. Dr. Cason offered as correction 40 σ for the time to close the eye to the point necessary to record with his device, 10 σ for the latent time of the inductorium, 12 σ for the latent time of the relay or telegraph sounder, and 4 σ for the sound to travel through the air. After stating these figures, he accepts 50 σ as the round number to be used in correction. This brings the latencies for the five subjects to 45, 53, 71, 77, and 81 σ, values which are still rather high in view of the range of 40 to 67 σ for reflexes to shock found by Bernstein. It should be pointed out that it is impossible to secure accurate latency measures through such corrections, because of the variability in the velocity of closure; Bernstein reports recruitment times for various responses of from 20 to 95 σ (**1**, 182). I have accepted the 50 σ correction as having Dr. Cason's full endorsement.

The conditioned responses reported in Table I were obtained by presenting the sound alone in the midst of a series of simultaneous sound-shock stimulations. The voluntary responses were obtained to the same sound *after* the conditioning experiment, under instruc-

TABLE I

FREQUENCY DISTRIBUTIONS OF THE LATENCY OF REFLEXES TO SOUND-SHOCK, VOLUNTARY RESPONSES TO SOUND, AND CONDITIONED REACTIONS TO SOUND FROM DR. CASON'S RAW DATA (2, TABLES II, III, IV, VIII, IX)

Corrected Latency (Reported Latency Minus 50 σ)	Frequency of Responses at Each Latency			Remarks
	Reflexes to Sound-Shock	Voluntary Responses to Sound	Conditioned Reactions to Sound	
−20 to −10.1			1	C-R's precede stimulus.
−10 to − 0.1			8	
0− 9.9			3	C-R's at less than reflex latency.
10− 19.9			0	
20− 29.9			5	
30− 39.9	34		4	
40− 49.9	74		1	
50− 59.9	65		7	
60− 69.9	51		7	
70− 79.9	34		10	C-R mode at reflex latency.
80− 89.9	23	1	10	
90− 99.9	6	4	7	
100–119.9	7	18	21	
120–139.9	5	14	24	
140–159.9		19	36	Mode of both C-R and vol.
160–179.9		18	14	
180–199.9		17	13	
200–219.9		18	8	
220–239.9		11	5	
240–259.9		8	4	
260–279.9		6	3	
280–299.9		3	5	
300–319.9		4	1	
320–339.9		1	2	
340–359.9		3	0	
360–379.9		2	2	
380–399.9		0	1	
400–419.9		2	1	
420–439.9		0		
440–459.9		0		
460–479.9		0		
480–499.9		0		
500–519.9		1		

Since Dr. Cason did not use all reported reactions in calculating mean values, only those reactions are included in the table which entered into his calculations.

tions to wink as quickly as possible. No ready signal was used in either case. The circumstances were as comparable as possible, and according to Dr. Cason: "Any factors which tended to make the voluntary reaction time *slow* had exactly the same chance I think of making the conditioned reaction times *slow*" (**2**, 187). Do his data bear out this assertion?

The most important feature in which the distributions of conditioned and voluntary responses in Table I differ is in the number of conditioned responses of extremely low latency. It is remarkable that some conditioned responses show much lower latencies than responses to the sound-shock combination. The sound and shock occurred simultaneously; the subject should not have known whether sound-shock or sound alone was to follow; there is therefore no immediately apparent reason why these very short latency responses should have been confined to conditioned reactions. Examination of the responses of the five subjects whose reactions are included in Table I shows the following results for the latency of conditioned responses as compared with *the minimum latency of any single reported reflex* to sound-shock for that subject: Subject A. R. has 10 out of 79 C-R's of latency lower than any single one of the 39 reported reflexes to the sound-shock combination; Subject E. B. gave 6 out of 22 C-R's of latency less than any of the 34 reflexes to sound-shock; Subject B. C. has 2 out of 35 C-R's of latency shorter than the shortest of 41 reflexes; Subject M. A. has 4 of 31 C-R's below the lower limit of 96 reflexes. Only Subject M. W., with the lowest latency reflexes, has no C-R's below the shortest reflex latency. Out of a total of 203 C-R's reported for these subjects, 22 are therefore of latencies which are unreasonably low apart from any preconception as to what the latency of conditioned reactions ought to be. The unreasonableness is emphasized by the fact that of these at least 9 *precede the stimulus*, while not a single reflex, and not a single voluntary response is reported to precede the stimulus. This seems to show that the series are not comparable, and interpretations based upon their differences in mean latency are of doubtful validity.

Such differences do not occur by chance, and the specific conditions which caused them are highly important. There are at least three possibilities. (1) The intermittent blinks within and between trials (**2**, 167) can account for the conditioned reactions which preceded the stimulus and occurred at reaction-times of less than reflex latencies. During the voluntary series the subjects were under instructions and may well have inhibited the incidental winking.

Although Dr. Cason has examined and rejected this possibility (**2**, 169), I still believe it to be important. This leaves the absence of low-latency responses during the sound-shock trials to be accounted for. (2) Some change may have been associated with the presentation of the sound alone during the test trials of the conditioning series which resulted in the extremely low-latency responses being confined to this one situation. A cue might have been present; a delay might have permitted temporal conditioning to be revealed only on these test trials. Dr. Cason's massing of trials made temporal conditioning a real possibility. (3) Because of certain standards with respect to reflexes and voluntary responses, atypical responses might have been discarded. Since no such standards existed for conditioned responses, all might have been included. I do not advance this suggestion seriously, for I doubt if Dr. Cason would have made the statistical error which this last suggestion implies. The other possibilities are mere conjecture; in any case the fundamental fact remains that the early responses appeared *only* among the conditioned reactions. It is impossible to correct for these early responses, for there is no way of knowing to what extent all conditioned responses may have been affected by the same factors which occasionally made them occur before the stimulus.

I gave another suggestion in my monograph (**9**, footnote 19, page 37), which, were it not for the early responses, might have explained why Dr. Cason found the latency of conditioned responses intermediate between reflexes and voluntary reactions. I suggested that he may have measured *both* reflexes to sound and conditioned responses of longer latency, and in that case he would have found a mean value between these two. There is a suggestion of this in Table I, which shows a mode for conditioned responses at approximately reflex latency, and another mode coinciding with that for voluntary responses. This interpretation becomes more plausible on the basis of Bernstein's experiments with stimuli similar to Dr. Cason's. Bernstein had objective records of the course of contraction, and could distinguish between modified reflexes to sound and longer latency conditioned reactions; had he not been able to do so, his results might have been more similar to Dr. Cason's.

The foregoing analysis suggests that the conditioned responses which Dr. Cason measured may have been composed of (*a*) incidental winks, some of which preceded the stimulus, some of which followed at less than reflex latency; (*b*) reflexes to sound; (*c*) conditioned reactions of voluntary latency. The presence of responses of

type (*a*) is unquestionable, but his method of securing data gave no way of distinguishing between the other types of response. Had he used another method, such as Dodge's which he recommended for accurate measurements (**2**, 157), he might also have distinguished between responses (*b*) and (*c*).

I am interested only in arriving at the truth of these matters, and I shall gladly admit that I have been mistaken if Dr. Cason will correct the misapprehensions under which I am laboring. My interpretation of Table I is that the data are unsuitable as a basis for comparing the latency of conditioned and voluntary processes. It is on the basis of these data that Dr. Cason's study stands in disagreement with the other studies which present data relevant to the problem of the latency of conditioned responses.

Summary

(1) I have attempted to show that Dr. Cason in his criticism of my study has failed to account for our chief point of disagreement: the latency of conditioned responses as compared with the latency of voluntary responses.

(2) In Dr. Cason's study, responses of extremely low latency (some preceding the stimulus, some of latency less than that of the shortest reflex response) occurred *only* among the conditioned reactions to sound and entered into the calculations. Such short-latency responses *never* appeared among the responses of reflex nature to the sound-shock combination, or in voluntary responses to the same sound. I have expressed the belief, therefore, that the data are unsuitable for a comparison of the latency of conditioned and voluntary responses, and, in consequence, that Dr. Cason's results are indeterminate.

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